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CLINICAL LECTURES ON SURGICAL SUBJECTS

DELIVERED IN

UNIVERSITY COLLEGE HOSPITAL

BY

CHRISTOPHER HEATH
PRESIDENT OF THE ROYAL COLLEGE OF SURGEONS OF ENGLAND
HOLME PROFESSOR OF CLINICAL SURGERY IN UNIVERSITY
COLLEGE, LONDON

SECOND EDITION

LONDON
J. & A. CHURCHILL
11 NEW BURLINGTON STREET
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TO

MY PREDECESSOR IN THE CHAIR OF
CLINICAL SURGERY

SIR JOHN ERIC ERICHSEN, BART., F.R.S.
PRESIDENT OF UNIVERSITY COLLEGE

I DEDICATE THIS VOLUME

AS A SMALL TOKEN OF MY ADMIRATION FOR
HIM PROFESSIONALLY

AND

OF MY SINCERE PERSONAL REGARD FOR HIM
AS A FRIEND

PREFACE TO THE SECOND EDITION.

THE following lectures, which form a small part of those delivered by me during the last twenty years, were reported by a shorthand writer, and were published from time to time in various medical journals. I have thought it well to collect them, and to publish them without attempting to alter their form, or to modify the views contained in them. I trust they may prove of interest to those who heard them, and possibly to others.

36 CAVENDISH SQUARE,
September 1895.

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ON

SURGICAL DIAGNOSIS.

Gentlemen,—As the Holme Professor of Clinical Surgery, it is my special duty to instruct you to the best of my ability in the diagnosis and treatment of surgical disease and injury. Much of my teaching must necessarily be at the bedside; but the clinical lecture which I shall deliver each Monday in this theatre will give me the opportunity of commenting more freely upon the cases you will find in the wards, than would be convenient before the patients themselves. The weekly clinical examination of actual patients by yourselves, or, at least, by the seniors of the class in your presence each Friday, will also, I hope, help to train you in the diagnosis of surgical cases; and I propose to devote this lecture to giving you some practical hints on the subject.

Many surgical injuries and affections are so obvious that no great skill is required for their recognition; but, on the other hand, similar out-ward appearances may depend upon very different causes; and it is here that the surgeon's skill in diagnosis gives him the advantage in prescribing appropriate treatment. Symptoms may be conveniently divided into "objective" and "subjective"; the former being those open to the observation of the surgeon, the latter those experienced only by the patient. It is to the objective symptoms of disease and injury that I propose to direct your attention to-day; for I do not mean, on this occasion, to bring a patient before you, but simply a living model with no special ailment, to illustrate some points in diagnosis.

It must be your great object to cultivate your eye, hand, and ear; and you cannot safely omit the use of any one of the three. Practically, one uses, as a rule, the ear first; because you are bound to hear something of the patient's own account of his ailment, in order to know to what part of his frame you are to direct your attention; though you must not put too much trust in a patient's own account, and still less in his diagnosis of his own malady. Thus, pain referred to the knee frequently depends upon disease of the hip, and pain in the loins from supposed kidney disease, upon stricture of the urethra. The eye will take in many of the leading features of a case at a glance, such as the general aspect of the patient, his mode of walking or standing, &c.; and, when the affected part is exposed to view, it will in many cases, if sufficiently practised, complete the diagnosis. Let me warn you, however, against "jumping to conclusions" too rapidly; for those who are foolish enough to pride themselves on rapidity in diagnosis not unfrequently make mistakes, which a little more time would have enabled them to correct. You can no more afford to leave out steps when tracing the nature of a case by progressive induction, than you can venture to omit one or more tests when examining a complicated fluid in the chemical laboratory. Lastly comes the hand, which may require to be fitted with various instruments for special purposes; but which, in its simplicity, is the surgeon's most powerful ally if properly educated. The much-talked-of *tactus eruditus* is to be acquired by all who will take the trouble to practise their fingers in the manipulation of healthy and diseased subjects, and will, at the same time, use their mental powers. For, of course, no amount of examination of a patient will tell you what is the matter with him, unless you elaborate the facts thus ascertained in a mind which has been trained by previous study to comprehend the importance of the several symptoms; and this is the reason why, at first, beginners must be at a disadvantage, lacking the experience necessary for a clear appreciation of symptoms. You will find, however, that with a little help from the surgeon, you will soon begin to see the "why" and the "wherefore" of diagnosis and treatment; and let me beg of you not to be afraid of asking questions—so long, at least, as the questions are put for the purpose of increasing, and not of displaying, your own amount of information. One pitfall I must just allude to: and that is the tendency to over-elaboration of a diagnosis which besets some men, and seems to stick to them through life. They never can take the plain obvious facts of a case, and from them draw a conclusion; but must dig and delve so deeply that they sometimes get, so to speak, "out of their own depth," and never come up again so far as any utility to the patient in question is concerned. Superficial carelessness is bad; but too great profundity is not much better.

And now I bring before you a man with no special deformity or ailment, in order that I may be able to show you a few noteworthy points which you can readily appreciate at a little distance, and which will assist you in studying disease and injury in the wards. With his back towards us, you have the opportunity of examining a healthy spine; and you may notice that, while the spinous processes are readily visible in the dorsal region (and particularly when the arms are folded), they are not so visible in the lumbar, and still less so in the cervical, region, where they are covered by muscles and ligaments, the seventh, or *vertebra prominens*, being the only one really seen or felt. Here, in a healthy adult, we have the average anterior and posterior curves in the lumbar and dorsal regions; but you must remember that in young children the spine is nearly straight, while in disease we may have great exaggeration of either curve. Thus, in the back, we find *kyphosis*, or angular curvature, the result of caries of the vertebræ; while in the loins we have *lordosis*, an exaggeration of the healthy curve, and generally connected with old hip-disease. Of the latter, I now place an example before you; and, of the former, there is an instance in No. 1 Ward, in a man whose foot I removed last week.

The model is now standing at "attention," with his knees straight; consequently, the two sides of his pelvis are perfectly even; and you see that a tape carried between corresponding points on the two sides is horizontal. Let us now make him "stand at ease," with the left knee bent and the foot slightly advanced, and you see that at once the left side of the pelvis is lowered. But this is not all. Corresponding with the obliquity of the pelvis, we have a lateral deviation of the spine to the left in the lumbar region; and if the man could sufficiently relax his muscles at the moment, we should have a curve in the opposite direction—to the right—in the dorsal region. By placing a book beneath the right heel, and thus increasing the obliquity of the pelvis, I exaggerate the lumbar curve; or, by tilting the pelvis in the opposite direction, I could throw the spine over to the opposite side. Of course, the same thing holds good if the patient be seated instead of standing; for, by tilting his seat, we are able to produce a marked lumbar and a certain amount of dorsal curve at will. Fortunately, we have here to-day also a case of old hip-disease, in whom the obliquity of the pelvis is well-marked, and the resulting twist of the spine better seen than in the healthy subject. You see, then, how important it is in any case of lateral curvature to ascertain whether it depends upon some obliquity of the pelvis (from atrophy of one leg or old hip-disease), or upon other causes; and you also see what effect upon an existing curve may be produced, as has been well pointed out by Mr. Barwell, by raising the side of the pelvis by means of a thickened sole or a sloping seat.

Turning, now, to the neck and shoulder, I pass my finger along the clavicle, which is subcutaneous, and shows its curves well enough in a thin muscular subject. The notch between the clavicles is important in connection with aneurisms of the great vessels of the neck; but the inner end of the bone is very rarely dislocated. except by extreme violence. The outer end of the clavicle is continuous with the acromion process, and I now run the chalk along them ; but it may be dislocated (as we have lately seen), and then the flattened end of the bone is readily felt beneath the skin. If I make the man swing his arm round, you will be able to appreciate, better than you perhaps have hitherto done, the great range of motion in the sterno-clavicular articulation, which, in fact, admits of "circumduction," and has a most important relation to the movements of the arm.

There is no joint, I suppose, about which mistakes are more frequently made than the shoulder. An "obscure injury about the shoulder" has often damaged a surgeon's reputation, because he has not sufficiently studied the anatomy of the part to be quite sure of his diagnosis and treatment. The chalk-line I have already made marks the bony arch formed by the clavicle and acromion ; but you will notice that the head of the humerus projects beyond this in front, and gives the roundness to the healthy shoulder. There is a hollow immediately behind the head of the humerus and below the prominent acromion ; and another in front, to the inner side of the head, in which the coracoid process can be more or less distinctly felt, according to the muscularity of the subject. In the healthy subject, there is just room to lay the finger between the coracoid process and the head of the bone. When the head of the humerus is *dislocated*, the roundness of the shoulder is lost, and the acromion stands out prominently beneath the skin, with a depression below it; while the head can be felt in some unnatural position, and out of its proper relation to the coracoid process. The direction of the whole limb is altered too; so that the patient cannot place his hand on the opposite shoulder with the elbow touching the chest-wall, as he can in health. In a *fracture* of the neck of the humerus, the roundness of the shoulder is not lost; but there is a depression below the head, which does not move when the arm is rotated, unless, indeed, the fracture be impacted. The two accidents may be combined; but this is a rare complication.

When I turn the model with his back to you, and make him raise his arm, you will appreciate how important the movements of the scapula are. The deltoid alone can only raise the arm to a right angle with the trunk; and the subsequent elevation of the limb depends upon a rotation of the scapula on the ribs, so that the angle comes forward to the margin of the axilla. The scapula is held in its place by the muscles passing from the spine to its base, and by the serratus magnus. If these be paralysed, the scapula falls forward, and the power of the arm is greatly lost.

Let us pass on to the elbow. With the forearm bent, you see at once the prominent olecranon process of the ulna. When this is broken off by falls on the elbow, the fragment is drawn up by the triceps, and an interval can be felt between it and the bone, which, however, is rapidly filled up by the effusion immediately following any injury to the elbow-joint. A much more common result of a fall on the elbow, however, is an injury to the bursa which lies between the subcutaneous triangle of the olecranon and the skin. In health, this bursa cannot be recognised, except in individuals whose occupation has produced enlargement of it—*e.g.*, miners or sweeps; for though it is technically called the "student's bursa," I must confess to have never seen an enlargement of it due to long poring over medical treatises. When the bursa is inflamed, it causes a swelling over the olecranon, which is evidently quite superficial, and does not mask the general outline of the joint, as would be the case were the effusion in the articulation.

The condyles of the humerus are readily felt in this thin arm; and you will notice that the internal is the more prominent, and stands out beneath the skin. Immediately behind it is the groove in which the ulnar nerve lies, as you may ascertain for yourselves by "twanging" it; and then comes the olecranon, with only just space for the nerve between the bones. This close relation of these two prominent points of bone is of great service in the diagnosis of dislocation of the forearm; for, so long as they lie close together, the deformity cannot be caused by a dislocation of the ulna. The outer condyle is more rounded; and immediately below it is the head of the radius, which can be felt rotating when the forearm is pronated or supinated. It is not often dislocated, for it is firmly held by ligaments; but it is sometimes thrown forward, and then prevents complete flexion of the forearm, by coming against the front of the lower end of the humerus.

At the wrist, you may notice that the end of the radius is lower than that of the ulna, and that the styloid processes of both bones are to be felt. Dislocation of the carpus from the radius is rare; but separation of the lower epiphysis of the radius is by no means uncommon in young persons, and is sometimes mistaken for dislocation. The triangular fibro-cartilage which binds the radius and ulna together at the wrist, sometimes becomes displaced in children who are dragged forcibly by the hand, and then the little patient cannot supinate the forearm and hand; but if you hold the hand firmly, and then supinate, you hear a slight click, and all is well. It is well always to bear in mind that, in supination, the bones of the forearm are parallel, and in pronation are across one another; so that supination is the posture in which fractured bones of the forearm should be set, although it is convenient, when the bones are firmly held by splints, to turn the thumb upwards—*i.e.*, to place the hand midway between pronation and supination.

In the hand, I need only point out that the superficial palmar arch does not correspond precisely to any one of the lines in the skin of the palm; but its convexity reaches generally to the middle one of the three, while the deep arch is much nearer the wrist. The bifurcation of the digital arteries is between the heads of the metacarpal bones, and about midway between the line to which I have referred and the web of the fingers; so that incisions should always be made in the line of the fingers, and not between them. As you may have, early in your career, to amputate a crushed finger, I would remind you, also, that the prominence of each knuckle is formed by the proximal bone of the articulation; and that the joint through which the knife must pass is below this in every case.

Descending to the groin, you see that the fold of the groin corresponds to Poupart's ligament; and an inguinal hernia is above, while a femoral hernia is below, this line—at least at first. Of course, a large inguinal hernia will descend into the scrotum, and a large femoral hernia may turn up over Poupart's ligament, and closely simulate the inguinal variety. But you ought to have little difficulty in distinguishing them if you will invaginate a piece of scrotum (or labium in the female) on the forefinger, as you see me doing, so as to carry the finger into the external abdominal ring. This will enable you to settle at once whether the protrusion has taken place through the inguinal canal or not. Let me also remind you always to ascertain the presence of two testicles in the scrotum; for an undescended testis may closely resemble a hernia, especially if inflamed.

The pelvis is so firmly bound together that a dislocation of one of the innominate bones can only be produced by extreme violence; but disease of the sacro-iliac joint is by no means uncommon, though often overlooked; and one leading symptom is lameness, which is erroneously referred to the hip-joint. But if I make the man before us stand on one leg, you will see at once how the whole weight of the body falls upon the corresponding sacro-iliac joint while he is throwing the other leg forward; and it is this which gives rise to the pain, and prevents the walking of a sufferer from sacro-iliac disease. Taking this man, who has a healthy pelvis, I may compress his two innominate bones without giving any pain; but a woman who has recently had a severe labour, and has, perhaps, incipient sacro-iliac mischief, will cry out at any such rough treatment, though the steady support of a good pelvic belt would give her great comfort, and restore her powers of locomotion.

The hip-joint in health is freely movable in all directions, as you see, and independently of the pelvis ; but, the moment the joint is inflamed, the muscles instinctively contract, and fix the joint to some extent ; and then, when the limb is moved, it carries the pelvis with it. This man's back is naturally more curved than a child's ; but even in him, if I make him lie flat on the table, you will see that the thigh can be fully extended without tilting up the pelvis, and so increasing the curve of the lumbar spine. In a child, whose back is naturally flat on the table, the effect of early hip-disease is readily seen (as I have frequently demonstrated to many of you) ; for the attempt to bring the thigh down, at once elevates the pelvis, and causes that curvature of the lumbar spine which, in old hip-disease, becomes permanent.

The prominence of the great trochanter will vary in different individuals, according to the muscularity of the buttock and the length of the neck of the femur ; and it is important, therefore, to compare the two sides in every case of suspected disease or injury. The head of the femur can be indistinctly felt on deep pressure, either in front of or behind the trochanter ; and, in health, the two move together ; for if the trochanter can be freely moved by rotating the femur without affecting the head of the bone, it is clear that the neck must be broken. The length of the neck will very much affect the power of rotating the limb; thus, if the neck be shortened, either by an impacted fracture or the absorption of old age, the arc in which the upper part of the thigh moves will be found to be much smaller than in health. When I stretch a tape from the anterior superior spine of the ilium to the tuberosity of the ischium, you see that, in health, it touches the top of the greater trochanter; now, if the bone were dislocated or the neck broken, the trochanter would be above or below this line, which is commonly called Nélaton's line.

When the knee is extended, you see the patella forming a prominence in front of the femur; but, when the joint is flexed, it sinks into the hollow between the condyles. With the leg fully extended and the muscles relaxed, there is, as you can prove in your own limbs, considerable lateral movement of the patella possible in the healthy joint; and the mistake is sometimes made of attributing this mobility to the presence of synovial effusion. When fluid is poured into the knee-joint, however, not only does the patella float so as to be freely movable in any position of the limb, but the synovial pouches on each side of and above the patella are distended, and give the characteristic roundness to the knee. If one kneels down on a flat surface, and particularly if the body be bent forward, as in scrubbing a floor, the patella and the bursa between it and the skin are exposed to considerable pressure; and hence the chronic enlargement of that bursa, termed "housemaid's knee," which causes a globular swelling in front of the joint, altogether different from that of effusion. Kneelers on hassocks or foot-boards do not run any risk of the housemaid's fate; for the pressure in their case comes on the tubercle of the tibia, and the bursa between it and the *ligamentum patellæ* would suffer if the pressure were sufficiently prolonged. In falling with the knee bent, the patella reaches the ground first and receives the force of the impact, which may simply bruise or cut open the bursa; or, if very severe, may "star" the patella itself. The transverse fracture of the bone is produced through the effort of the patient to save himself, by which the great extensor muscles catch the bone across the condyles, and either break it or rupture the ligament; then the upper fragment is drawn up in front of the femur, and a space is left in which the condyles can sometimes be felt, as in a patient recently in the wards.

With the knee flexed, the rounded outlines of the condyles can be readily felt resting on the top of the tibia; and a little distance below the outer condyle can be seen the head of the fibula —which bone, let me remind you, does not enter into the formation of the knee-joint. The existence of the semilunar fibro-cartilages between the femur and tibia is hardly appreciable in health; but their existence must not be forgotten, as occasionally, in violent wrenches of the knee, one of them becomes displaced, giving rise to extreme pain and inability to use the joint, which are most satisfactorily treated (as also are dislocations of the patella) by a little of that judicious violence for which "bone-setters" have a reputation.

At the ankle, we see at once the prominences of the two malleoli, between which the astragalus fits closely when the foot is at right angles to the leg, less so when the foot is pointed; so that, in this position, some amount of lateral movement of the foot is possible. The fibula is altogether posterior to the tibia, and its malleolus is longer than the internal. The lower third of the fibula is subcutaneous, and its fracture (Pott's fracture) is therefore readily recognised. The tendons of the various muscles surround the ankle-joint; but the only one to which I need call your attention is the *tendo Achillis* at the back, in which, when ruptured, the division is readily both felt and seen. When I flex the knee and point the toes, you can see how completely the muscles of the calf are relaxed; and this is an important point in the treatment of a divided tendon, or of a dislocation of the foot, or of an oblique fracture of the tibia.

The prominences of the foot are chiefly important as guides to the amputations; thus, the tuberosity of the scaphoid on the inner side marks the transverse tarsal joint, or site of Chopart's amputation; whilst the base of the first metatarsal on the inner and the prominent fifth metatarsal bone on the outer side, mark the position of Hey's amputation. The metatarso-phalangeal joint of the great toe is not unfrequently diseased through gout, or the pressure of boots which have developed a bunion; and other toes are not unfrequently deformed from the same cause. The only surgical point with regard to the toes which I need mention is that the base of the first phalanx is more expanded and more deeply placed than young operators are apt to imagine.

I have thus run briefly through on the living body the more salient points which it is important for you to recognise thoroughly in health, before attempting to treat disease; and, in future lectures, I shall have to direct your attention to matters regarding which the knowledge acquired to-day will be of service in enabling you to recognise deviations from the standard of health.

ON

SACRO-ILIAC DISEASE.

The disease of which I am about to speak to-day, gentlemen, is one which, perhaps, is not very common; but in fact it is only of late years that it has been recognised. It may be of some interest in the history of the disease for you to know that the first systematic description of it was given in this hospital by Mr. Erichsen, in a clinical lecture which was published in the *Lancet*, in January 1859. Since that time, of course, the attention of surgeons has been drawn to the disease; and, therefore, you will find that by most modern writers mention is made of it. I have had a few cases of sacro-iliac disease in the hospital lately.

The first case was in a man who was in the hospital last year. He was admitted in May into the medical wards, under the care of Dr. Reynolds, for sciatica; but, an abscess developing about the pelvis, I was asked to see him some weeks afterwards; and I then diagnosed that he was suffering from sacro-iliac disease, so that my notes begin from that date, and I will give you a brief sketch of the history of the case from the report of the ward-clerk.

The patient had been a soldier, and had been to India, where he had had dysentery, dengue fever, ague, &c. While in India, he also had an abscess in the perinæum.

Present Illness.—About the end of January or the beginning of February (the patient does not recollect the exact date), he was going upstairs to bed, after having been out for a short walk, and was seized with a sudden and sharp pain at the back of the left hip, and was unable to move for some few minutes, after which time he managed to crawl up to bed, where he remained for two days. The same evening he sent for a doctor, who gave him some medicine, and a lotion to apply, and told him on the third day that he would be better if he could get about a little. He left his bed that day, and was able to move from one place to another with the aid of two sticks.

On May 5th, 1875, he was admitted, under the care of Dr. Reynolds, with sciatica. The pain was so great that he was ordered hypodermic injections of morphia every night. The patient complained of pain at a point on a level with and behind the great trochanter. The nature of the pain was continuous, with an occasional shooting. There was great tenderness downwards in the direction of the great sciatic nerve, which tenderness reached also round the loins. The patient could not stand well on the left leg, which seemed weaker than the right. There was a slight swelling in the position of the left sacro-iliac articulation, which was first noticed on July 5th. The swelling was regular, ovoid, fluctuating, and very tender on pressure. There was a slight curvature of the lumbar spine to the left side. The patient was thin and considerably emaciated, pale, and anæmic. He lay on the right side, with the left hip flexed at a right angle, because this position was the most comfortable for him; he could, however, also lie on the left side. There was no irregularity of the spine; but on the left side, opposite the two lower lumbar vertebræ and the upper part of the sacrum, there was very great tenderness; and, immediately to the left side of this, there was a swelling extending outwards four inches, and three and a half inches from above down. Fluctuation was superficial, and there was considerable tenderness over the swelling. There was distinct impulse on coughing. The tumour was dull on percussion. A very slight tap on the crest of the left ilium caused very considerable pain, which he referred to a line corresponding to the left iliac synchondrosis. No abscess could be detected in the groin along the course of the iliacus muscle. The movements of the hips were free, and caused no pain, except when extreme flexion or rotation was practised, and then the pain was referred to the side of the sacrum. Tapping the great trochanter or the heel produced pain of the same kind. There was considerable wasting of the left lower limb. The patient could stand, and walk with the aid of two sticks. Standing on the right leg only could be managed without much difficulty, but it was almost impossible for the patient to stand on the left leg. On his admission into Ward 1, Mr. Heath ordered a broad bandage and plaster to be applied tightly to the hips, so as to keep the pelvis firm.

On July 18th, another swelling was noticed about the size of an egg, communicating with the first one. In this, as in the other, there was a distinct impulse on coughing.

July 24th. The swelling had increased in size. The pain in the back was still very bad.

July 27th. The patient was aspirated to-day, and about half an ounce of thick creamy pus was withdrawn. Collodion and lint were applied, and strapping.

July 30th. The swelling had increased in size, and was nearly the same as before.

August 19th. The patient was aspirated again to-day, and was discharged with a tight-fitting belt.

Now, here we have a clear history of the symptoms of the man at the time he was admitted; and let me particularly draw your attention to the fact of his being unable to stand on the affected side, though you must remember that we have other diseases which prevent the patient from standing on the affected side. He had wasting of the left hip and thigh, which was simply due to his not using the limb. Any limb will waste if you do not use it, so you must not lay too great stress upon atrophy as a sign of disease.

This case was, comparatively, easy to diagnose. There was a well-marked abscess at the time when I saw the patient, and on pressing the two ilia together the man complained of considerable pain over the sacro-iliac joint. The abscess was aspirated more than once, and I had the man fitted with a belt, and eventually he was able to leave the hospital in a fairly comfortable state; so, although his stay in the hospital was rather a long one, he went out in a favourable condition. I have not heard anything of him since, but I dare say I should have done so if he had not gone on well.

The second case was that of a woman, who was admitted for the second time last year. She was under my care three years ago, because then she had a swelling of the *right* iliac fossa, which was aspirated, and thirteen ounces of fluid were withdrawn. Although I tested her for sacro-iliac disease then, I could not detect any. She went out, and remained without any particular ailment until she was re-admitted. And then there is this curious point in this case—viz., there was a history of previous illness ten or twelve years ago, when she suffered from fistula, and two pieces of bone were taken away.

Condition on Admission, September 26th, 1875. — In the left iliac fossa, just above Poupart's ligament and over the iliac artery, was a very hard, round swelling, of the size of a small orange. It was fixed to the iliac fossa and did not pulsate. The upper edge was distinctly rounded, and the margin could be felt all round. It was very tender. The patient complained of a sharp cutting pain over the abdomen and down the thigh. She was not anæmic, and was fairly nourished. There was no tenderness over the spine, nor at the sacro-iliac synchondrosis.

I tested her for tenderness, imagining that there might be caries and abscess connected with diseased vertebræ ; and I may say that the case was complicated by her having a fibrous tumour of the uterus, which I got Dr. Williams to examine.

October 21st. Outside the lump above described, there was a soft, rounded, distinctly fluctuating swelling, which had gradually increased during the last three or four days. In the afternoon, I drew off seven and a half ounces of thick greenish pus, without odour. She had some little fever after that.

November 27th. I again aspirated, and drew off about eight ounces of pus from the left iliac fossa.

December 15th. The patient complained of pain in the back; and said that, when she put her heel to the ground, she felt as if a knife were run into her over the sacro-iliac synchondrosis. This was the first time she complained of the characteristic pain of sacro-iliac mischief.

January 28th. The patient had the abscess in the left iliac fossa aspirated, and eight and a half ounces of pus, with a little blood, were withdrawn.

February 10th. Just inside the anterior superior iliac spine, on the left side, was a swelling as large as a pigeon's egg, fluctuating, becoming tense on extending, and relaxed on flexing the thigh. By pressure in the iliac fossa, the swelling was made decidedly tense. She complained of startings, pain, grating in walking, and had very little control over her limbs. After that, she had a pelvic band fitted on, consisting of two bands passing round the pelvis and fastening on a broad pad in front of the pubes, with elastic bands passing round the thighs, and fastening to the pad in front. Subsequently, she was discharged without any further tapping, and went to Walton.

She applied here again last July, wishing to be tapped; for the abscess had refilled. Finding that I was going out of town, she said she would not come in then, but would wait till I returned. When I came back in September, she called here to tell me that the abscess had entirely disappeared; that it had discharged from the rectum. I examined her, and there was nothing to be felt. Fortunately for her, no gas found its way from the rectum into the abscess.

The third case is one that is now in the ward. The patient came here because she was lame, and we were told nothing more than that; and being exceedingly deaf, it was rather difficult to make out anything from her at the time. I examined the patient in her clothes, and found nothing wrong with the hip-joint, and did not detect anything more then; but Mr. Gould called my attention next day to a considerable abscess in the left iliac fossa, which abscess, somewhat unusually I think, took its course down the psoas muscle. The history of this last case is rather obscure, but it is this.

Lucretia A——, aged twenty-five, single woman, was admitted September 18th, 1876. For nearly six months past she had felt pain in her left groin, and also across her loins, especially after standing or walking for any length of time. The pain was always greatest towards evening, but was relieved by lying down. Three months ago she noticed a swelling on the inner side of her left thigh, which increased in size towards evening. She often felt pain right down her leg, and could not bear to walk far. She said her leg became stiff when she attempted doing so. She had never had rheumatism, nor been injured in any way that she was aware of. Two years ago, she said, her abdomen and legs were very much swollen, her bowels were confined for a week, and she passed no urine for a fortnight (?). She had been out of health for the last two or three years, but she did not complain of anything in particular.

Present State.—The patient complained of constant pain just above the left ilium, which prevented her from walking. Percussion of the trochanter or heel, and forcible rotation of the femur, were said to give pain. There was distinct fulness above Poupart's ligament, and outside the line of vessels, with distinct fluctuation transmitted from this to below Poupart's ligament outside the vessels; also fluctuation was distinctly felt on the inner side from the iliac fossa to the inner side of the vessels. The hip-joint could not be placed absolutely straight, and was kept very slightly flexed. There was no prominence of any vertebra. Percussion over the first and second lumbar vertebræ caused pain. She complained of pain on pressure on the ilia. There was no shortening of the limb.

October 8th. I aspirated, and about three ounces of creamy pus were withdrawn. What we have done in this case was to fit the patient with a belt and tap the abscess. The belt which I have fitted consists of two pads pressing on the pubes, the straps passing twice round the body, with perinæal bands which keep it from riding.

Now we will pass on to the symptoms and nature of the disease. Take a case of pain about the region of the hips, what are the symptoms which would lead you to conclude that it was sacro-iliac and not hip disease? First, motion is limited in hip disease, you cannot move the thigh freely because the muscles prevent it; but in sacro-iliac disease, if you take the precaution to fix the pelvis, you will find that you will be enabled to get all the movements of the hip. When the patient stands the diseased seems to be a little longer than the healthy side; and if you examine carefully, you will find that the lengthening is real lengthening; that the anterior superior spine is a little lower down than on the opposite side.

In looking over Professor Sayre's *Lectures on Orthopædic Surgery*, I find he has given a couple of drawings of sacro-iliac disease. The point they show, as you will see, is the wasting of the buttock, and that the patient's body is thrown to the healthy side, the object of the patient being to take the weight off the affected side. There is no doubt about this, and it is important to remember that the patient throws his body to the side opposite to the disease.

Abscess may form either in the iliac region, or behind in the sacral region, or you may have it taking the place of psoas abscess. It is very important that you should make out the nature of the abscess at once, because of the treatment. You know you may have abscess around the cæcum—peritypliltic abscess, from inflammation about the cæcum—and this occurs only on the right side; but on either side you may have abscess connected with the kidney, and it may be very difficult to tell by the symptoms whether it be due to sacro-iliac disease or to kidney. You may have abscess connected with hip-joint disease, and if you see the patient in the earlier stage you might mistake it for sacro-iliac disease. You may have abscess from diseased vertebræ in the lumbar region, or disease in the iliac fossa itself; and you must not conclude, therefore, that it is sacro-iliac disease because of the abscess alone. The best diagnostic point is the pain produced by firmly pressing the innominate bones together, or striking the two iliac crests with the balls of the thumbs. The patient suffering from sacro-iliac disease complains immediately, as a rule, and refers the pain to the affected joint. In some cases, however, pressing the parts together is not sufficient, and it is well then to try and draw the bones asunder, which will almost certainly show whether there is a tender joint or not.

Hilton has called attention to the fact, that the obturator nerve passing from the pelvis through the obturator foramen down the thigh, may cause pain on the inner side of the thigh during sacro-iliac mischief. I mention it here, and give his authority, because I do not happen to have a case to illustrate the point.

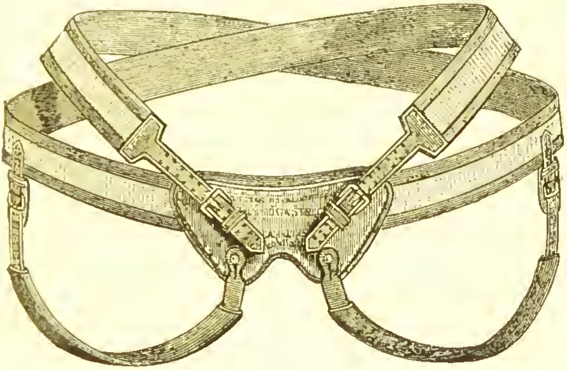
With regard to the cause of sacro-iliac disease, I may tell you that the first case I ever saw was in a young woman who had had children very rapidly; and I believe this is a very common cause. You will find that a weak woman has a child with a large head, and there is some difficulty in the head passing at the time of birth, and the mother is ailing and lame when she begins to get about.

Another cause is injury. Sayre says that he has met with it in children, but I have never seen a case. You may, however, trace it sometimes to direct injury, as we found in that man who had a fall upstairs. I am quite sure too I have seen pain in the joint connected with gout. Only a few weeks ago, a gentleman came to me for pain in the back, for which he had had the routine treatment for lumbago, and, as a last resource, I struck his pelvis, which caused pain. He was a very gouty subject, and I then knew that he was suffering from a little gout in that joint. I put him on alkaline remedies and gave him a belt, with complete relief.

Treatment.—The great point, I believe, is to secure perfect rest; and the reason I say so is, because I have proved it experimentally, and because I can show you here specimens of recovery from sacro-iliac disease. If we keep the joint quiet we get ankylosis; that is, growing together of the two surfaces of the joint.

I have here some bones showing the results of the disease. Here is a joint where there is perfect ankylosis; here is another, which is not quite ankylosed; and here is a third, where the sacro-iliac joint is not only ankylosed, but there is ankylosis of the bodies of some of the vertebræ as well. Seeing that we have here a method of cure, we must assist nature by keeping the joint perfectly at rest, and the difficulty is in doing that, because you will observe that you should not keep the hip quiet, but only the pelvis. Mr. Huxley made me a belt (Fig. 1), which answers very well. The pad over the pubes and the straps going round the pelvis keep it firm, and do not interfere with the patient's movements, but in applying it it is essential, first of all, to buckle up the two perinæal straps in order to keep the belt thoroughly in position.

Fig. 1.



Is any operative proceeding necessary? Mr. Erichsen does not approve of operative interference, but recommends simply opening the abscess. Bryant speaks of having more than once opened the sacro-iliac joint and removed diseased bone, but I have never met with a case requiring such treatment.* I see that Sayre, too, speaks somewhat in the same way. He says here: " If the disease has progressed, and suppuration becomes established, then, instead of cauterisation, lay the parts open freely, passing down until you have laid the joint bare, and if the probe detects dead bone anywhere, follow it up by freely laying the sinuses open, or make counter-openings, and gouge it out, for it must be removed before the patient can get well." He also recommends extension and counter-extension, but I do not think extension is necessary. Sayre also says, "If the case is one of long standing, and there is more or less deposit in and about the joint, or if the inflammation does not readily subside, application of the actual cautery directly over the sacro-iliac articulation will be of the greatest service"; but I cannot recommend the treatment from my own experience.

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* Since this lecture was delivered I have had occasion to lay open a sacro-iliac joint with the best results.

I believe the great secret of treatment is, to take the cases early, and not to treat them by extension and counter-extension, but to lock the parts thoroughly together. Our object is to give the patient rest, which I do not think he will get by pulling at the limbs, for it must be remembered that we are not dealing with a joint in which the articular surfaces can be separated. I have proceeded on this plan, and I must say that our cases have done very well.

ON

DISEASES OF THE TONGUE.

I PURPOSE to-day to speak of the diseases of the tongue. Of course the tongue is constantly looked at simply as an index of disease elsewhere; and, so far as that is concerned, I do not purpose to deal with it to-day, but to confine my observations to the surgical diseases of the tongue. I think you will find that the simplest way to consider these will be under three heads—namely, the inflammatory affections of the tongue, the ulcerations of the tongue, and, lastly, deposits in the tongue. Of course you will find the diseases of the tongue given differently by different authors, but, on the whole, I think that it is perhaps as convenient a way of putting it as any other.

First, as regards the inflammatory affections of the tongue. There is, of course, such a thing as acute glossitis, but it is very rare, and you have not had an example of it before you, that I know of, of late—at all events, not in my practice. Still you may meet with such a case, and it is well to know that patients are liable to a sudden great enlargement and inflammatory swelling of the tongue, which looks very formidable for the first few hours, and may sometimes prove dangerous to the patient's life by inducing suffocation. You may occasionally meet with it coming on apparently idiopathically, but more often it seems to be connected with the administration of mercury. A patient has been taking mercury for some reason; and he gets acute glossitis, it is difficult to say why. Under these circumstances you find the tongue enormously swollen, filling up the mouth of the patient and almost suffocating him, and naturally requiring to be relieved immediately. The treatment is extremely simple—by making an incision on each side of the median line of the tongue. Do not mind a little free bleeding. Bleeding relieves the affection, and then with a little fomentation and simple gargles the whole thing subsides in a few hours. It really is not a very important affection, although it looks very alarming when you first see it.

What I want to speak more particularly about to-day are the chronic affections of the tongue. We get chronic inflammation of the tongue from various causes. Perhaps one of the commonest is the form which is due to smoking, for there can be no question whatever that tobacco has a deleterious effect upon the mucous membrane of the tongue. If you must smoke, and I suppose in the present day it is difficult for any young man not to smoke, pray smoke mild tobacco, and do not use pipes which are saturated with oil, and which, perhaps, have been smoked by other people.

Now, with regard to the chronic glossitis which is produced by tobacco, you will find that authorities differ about the affection, but I agree with Mr. Barker in his essay in Holmes's *Surgery* that there is a condition of reddened tongue which is an early stage of chronic glossitis. When that has passed away you find a later stage where you have a development more or less complete (it varies, of course, in different patients) of a white patch. It may be simply a loss of epithelium—that is a common condition which is sometimes known as "glazed tongue"—but you frequently find in addition to that a development of white tissue in the tongue, of which you see a very good example in one of the illustrations in Mr. Butlin's book. We happen to have a patient who has been in No. 1 Ward lately for an anal affection. I have pointed him out to you when you have gone round, as a remarkably good example of what is known as the name of "leucoplakia" or "leucoma" of the tongue, and I will now bring him before you; and let me remind you that when you examine a tongue, the first thing always is to dry it. I will dry this man's tongue, and then you will see clearly a white patch, which is partly the cicatrisation resulting from an operation performed upon it.

Here we have a good example of the results of chronic glossitis, which at one time seemed likely to run into more serious disease. A growth developed in the white patch, which at one time looked exceedingly like epithelioma, so much so that the man was admitted into the hospital in 1886 with the view of having part of the tongue removed. It was not removed quite so promptly as it would have been but for a Fellowship examination which was coming off, and we wanted the patient to be shown. I postponed the operation, therefore, and during the fortnight or so that he was waiting here the tongue improved so much under simple treatment that I made up my mind that it could not be epithelioma, and accordingly I simply scraped away the disease which had sprung up in the white patch, or beneath it, and the result, as you see, is that the man is now well. He has a perfect tongue instead of having lost part of it, and it is quite clear that it was not a case of epithelioma developing in a patch, but was simply hypertrophy of the submucous tissues, the result of chronic inflammation. Let me at the same time warn you that there is no doubt whatever among those who have carefully observed these cases, that a patient who has a white patch is more liable to epithelioma than the patient who has not a white patch. I do not like to put it more strongly than that. I do not say that every such patient will have epithelioma, but his chances of having it are greater than those of a patient who has not a white patch.

The white patch may of course go on and develop into a sort of warty growth, as you see here in one or two of these pictures, or it may develop into a condition which is sometimes badly called psoriasis ; or again, it may develop by still greater irritation into what is sometimes called ichthyosis of the tongue. They are all degrees of the same disease. Ichthyosis was the name given by Mr. Hulke, who first called attention to it ; and it is undoubtedly much nearer epithelioma than the white patch. There is a drawing here in Mr. Clarke's book which illustrates it very well. It is a very good representation of a highly hypertrophied and roughened mucous membrane. An ichthyotic tongue is so near epithelioma that it is just a question whether you should remove the tongue at once, or wait until there are positive signs of epithelioma. I think myself, if I had a well-developed ichthyotic tongue, I should prefer to have it out at once rather than run any further risk.

Now let us take another class of case, where you have a chronic change in the epithelium due to syphilis. You may have a primary chancre. The early enlargement of the glands under the chin and jaw is very characteristic of chancre; but do not suppose that you will find that induration which we see so commonly on the penis, because you will not, any more than you do on the lip. The cases of primary chancre on the tongue are extremely rare; but you must bear in mind that a patient may most innocently get a primary sore upon the tongue from another. If a patient hands, from a mouth which has mucous tubercles in it, a pipe, which must necessarily have some syphilitic saliva upon it, to another person who has a little sore or crack on the tongue, it is conceivable that the tongue may become affected, and the patient may have a primary chancre. Much more common than that, but still rather rare, are mucous tubercles. You find that a patient who has syphilis in the system sometimes develops mucous tubercles about the tongue, particularly on the back part of the organ.

But the most common syphilitic affection of the tongue is another form of glossitis—chronic glossitis with cracks in the tongue—which is well recognised as the result of secondary syphilis. The picture in my hand shows it well. You see the loss of epithelium in the centre of the tongue, and round the sides you see a cracked, cicatrised tongue. Those deep cracks, sometimes running quite far into the tongue, are as characteristic as anything you can have of secondary syphilis. Then, if you go further and follow out syphilis, of course you may have gummata in the tongue; but I shall put these under the head of deposits, and I will say a word now with regard to the treatment of that which is so common an affection.

The question arises, in the case of such a tongue as I have described, Will you give mercury or iodide of potassium? Certainly, in a tongue like that you had better give small doses of mercury. It is true the patient may have had mercury before; but I find that these patients do better with small doses of mercury—which you can, if you like, combine with iodine in the form of biniodide—or you may give it separately as liquor hydrargyri perchloridi. The thing I lay most stress on, however, is the local application of mercury. These tongues do well if you pickle them in a mercurial lotion. It is of no use to tell the patients to use a mercurial mouth-wash and then spit it out. They should be told to hold it in their mouths ten minutes by the watch, and breathe through their noses, so as to get the tongue thoroughly pickled with the lotion. As to the strength of the lotion, I advise you to begin with a quarter of a grain (afterwards getting a little stronger) of perchloride of mercury, with a little honey to make it more palatable, in an ounce of water. If the process of "pickling" is repeated twice or three times a day, the effect produced on the tongue is astonishing; the ulcers will heal under the mercurial treatment, the tongue will cicatrise, and the patient will recover a perfectly useful tongue, in which there will be no risk, so far as I know, of any future development of epithelioma. It is long-standing diseases that develop into epithelioma; but if you cure the disease and make the tongue healthy, or comparatively healthy, the patient will be no more liable to epithelioma than any other person.

Now let us go on and take the forms of ulceration, which we have from three different sources. You may have the syphilitic ulceration, you may have the tubercular ulceration, and you may also have the epitheliomatous ulceration. Syphilitic ulceration I have already mentioned in connection with superficial inflammation of the tongue. It is well that you should remember that there is another form of ulceration connected with syphilis, that is, the ulcerated gumma. Some months ago we had in the hospital an old man, a foreigner, with very well-marked ulcerated gumma on one side of the tongue. I took him in because he could not be attended at home, and he got steadily better under good treatment. He was here a week or two ago to show his tongue, and I then called your attention to how very much it was improved. The treatment of these ulcerated gummata is of course a little different from that of the ulcers occurring in the earlier stage of syphilis. Where you have ulcerated gummata of the tongue, it would be unwise to push mercury. You give iodide of potassium, and improve the patient's health in every possible way; and if you use the mercurial wash, not stronger than a $\frac{1}{4}$ to a $\frac{1}{2}$ grain to the ounce, you certainly get those tongues better.

The tubercular ulcer of the tongue is not a very common thing, and you see it more often in private practice than in the hospital. It usually occurs at the tip of the tongue, and is, I believe, very often determined by irritation from the teeth; at all events, I have found that by covering the teeth with gutta-percha, so as to prevent the tongue being rubbed against the back of the teeth, I have been able to get these tongues very much better. Here is an example in Butlin's book of a well-marked ulcer of the tip of the tongue, and where you find an ulcer in that condition you will generally find some evidence of tubercle in the lungs or other parts of the body. An ulcer like that you must treat as you would treat an ordinary tubercular ulcer elsewhere—namely, by scraping away all the diseased tissue. You may or may not find tubercle bacilli in it. I am not prepared to say which is more common, but at all events you should scrape the ulcer, and then dress it with iodoform, and treat the patient with cod-liver oil and by general anti-tubercular remedies.

Then we come to the third form, which is the malignant or epitheliomatous ulcer. Now, why does a patient get epithelioma of the tongue? Invariably, I believe, from some irritation. Patients with perfectly healthy tongues do not get epithelioma. But if you have a patient who has a white patch beforehand, which is a little irritable, perhaps scratched by a tooth, and which is kept in a state of irritation by the constant application of tobacco juice, one can easily imagine *that* developing into epithelioma. But as far as my experience goes, the greater number of cases of epithelioma are simply due to the local irritation of a ragged tooth cutting into the tongue ; nothing has been done to relieve it, and then at last a development of epithelioma takes place. During the last winter I have seen two middle-aged gentlemen, both of whom had the very smallest patches of epithelioma that I have ever seen on the tongue from that cause. They both had sharp-pointed teeth, and they were conscious that those teeth had been irritating their tongues. They went on, however, until at last they began to find something wrong, and then they took advice. In both cases I cut out the piece, and it was simply mucous membrane that was affected by the disease, which did not go down into the muscular fibre of the tongue at all ; but still in both cases there was well-marked microscopic evidence of epithelioma. One may hope that by such an early removal as that, the patient may be completely relieved of the disease. Still, the great thing is to prevent the occurrence of such cases. I am happy to say that dentists are becoming better instructed every day ; they are more alive than they were to the importance of irritation caused by teeth, and they send on cases at once, when they find anything suspicious about the tongue, to medical men and surgeons to see what the matter is. Too often, however, patients themselves are content to let the thing go on; and then at last they come with a ragged ulcer, which is undoubtedly epitheliomatous.

I quite allow that the very early stage of epithelioma is a little difficult to recognise; but when the ulcer is well developed, and the induration at its base has become well formed, no one can hesitate for a moment. Here we have three specimens of epithelioma of the tongue. An epitheliomatous ulcer is not simply an ulcer, but an ulcer with a growth springing up at the edges of the base and forming a protrusion, sometimes a distinct protrusion, as in this particular case; but more often like this other case, an indurated base to the ulcer with well-marked granulations coming up into the cavity. Unfortunately, you have seen many of these cases, because we have a great many in the hospital at various times, and in hospital practice they do not come until very late, so late that generally the glands are enlarged. But in private practice we see them rather earlier, before the glands are enlarged, and I want particularly to impress upon you the importance of not waiting for the glands to become affected. It is very absurd that a surgeon should think to himself, "This cannot be cancer because there is no enlargement of the glands," and then deliberately wait and watch to see whether the glands become affected or not before he makes his diagnosis. That is all very well as regards diagnosis, but very bad as regards the patient, and the patient who is waited for in that way is badly treated, for you ought, if possible, to get rid of the cancer before the glands become affected.

Let me now say a word about diagnosis, because that is really the important point. How will you diagnose a case of epithelioma when you see it? In the first place by the position. It is more generally at the edge and side of the tongue than anywhere else, and the history is pretty conclusive. You will find that for weeks or months the patient has had some irritation about the tongue; he has had an ulcer which has too often been constantly irritated by the application of nitrate of silver, and has developed more and more until there is the characteristic growth in the base and edge of the ulcer which I have mentioned. Then there may be tenderness of the glands of the neck. The pain, too, is characteristic; the patient tells you that it shoots up into the ear. I always dread that phrase, and never prompt a patient to use it, but as sure as he does so it is a case of cancer of the tongue.

Lastly, we have the deposits in the tongue, which I have mentioned incidentally, and which are of two kinds, the epitheliomatous, which always follows the ulcer and extends beneath it, and the gumma, which develops, as a rule, in the central portion of the tongue, and is apt to break down and give rise to a formidable-looking ulcer, as already described. Unquestionably an ulcerated gumma may develop into epithelioma, and it is these mixed cases which apparently improve for a time under iodide of potassium, and yet prove malignant after all.

What, then, is to be done for cancer of the tongue? Undoubtedly to palliate these cases is merely to lose valuable time, and the so-called "cancer cures" are simply swindles on the public. Chian turpentine stands on a different footing altogether, and I have used it where I thought it right as an adjuvant to surgical treatment, but I confess that I have never seen any benefit from it whatever. I have even sent a patient on whom I had operated, and who had a return of the disease, by his own wish, to Dr. Clay, of Birmingham, that he might himself carry out the treatment; but the patient died under the treatment, just as he would have done without it, and in about the same time, without having received the slightest benefit. I do not think there is any specific cure for cancer, and the only chance for the patient is to get rid of it as early as possible.

That brings me to the question of operations on the tongue. Of course there are many, but I am not going to trouble you with them all; it will be sufficient to tell you what we do here at the present time. We have all come, in most cases, to removing the tongue with scissors. Years ago, as you will find if you look into the records of the hospital, the galvanic écraseur was used, but we gave it up because it led to secondary hæmorrhage. It answers well for the time, if the battery is in good order, and you can remove the tongue without bleeding; but you are apt to get secondary hæmorrhage a few days afterwards, when the slough comes away. After the galvanic écraseur we used the wire écraseur. The best way of using it is Mr. Baker's modification of the operation—namely, to split the tongue down the middle, to loosen it from all its attachments beneath, and then divide with the écraseur far back, taking one-half of the tongue at a time. Of course in many cases you remove only half; it is not necessary to remove the whole. We have most of us now given up that operation and gone to the intra-buccal operation with the scissors, generally known as Mr. Whitehead's operation. It is extremely simple. All you have to do is to gag the patient thoroughly—it is important to have a good gag; then, getting a firm hold of the tongue, with the scissors you divide the mucous membrane under the tongue and the muscles, and, drawing the tongue well forward, next divide it at the root, cutting steadily through till the lingual artery is exposed, when you can often pick it up before dividing it. Having done one side, you go to the other, if you remove the whole tongue, and treat it in exactly the same way. The result is that you get the tongue away as far back as you like with very little hæmorrhage.

Let me say, on the subject of hæmorrhage, that I myself introduced an improvement in the operation about the tongue some years ago, which I find most surgeons have adopted—namely, that when there is hæmorrhage, and you require to stop it, you simply put your finger down the patient's throat and hook up the base of the tongue and the hyoid bone. I found that out when assisting my colleagues in removing the tongue. We used to have occasionally sharpish hæmorrhage. It was my business to help to pick up the arteries, and I found that by putting the finger well down I could stop the bleeding, making the tongue bloodless, and pick up the arteries quickly, as you might pick up an artery in a limb.

But then supposing you have disease of the tongue with enlargement of the glands, should you or should you not operate upon them? For myself I am not particularly fond of removing glands, yet I am obliged to do it from time to time. Where I find a patient has an ulcer of the tongue which is distinctly cancerous, with infiltration in the submaxillary glands, I do not hesitate to make an incision and take the glands out. It becomes a little different when the glands of the neck are affected. In my experience, cases where the submaxillary lymphatic glands are not affected and the glands under the sterno-mastoid are affected are the worst possible cases. If you cut out one gland, you find very soon afterwards that another becomes affected; and those patients, as a rule, have very short lives. Cases where you can remove the glands under the jaw merely do fairly well, as well perhaps as any of these cases. Because, after all, you must remember that recurrence is almost invariable, and one hardly knows of a patient with *bonâ fide* epithelioma living more than two years. Then in these cases where the glands are affected you may have seen some of my colleagues do a very large operation, called Kocher's operation. You make an incision into the neck, turn up a large flap, and get down upon the glands; you then get hold of the tongue, and you can undoubtedly remove numerous glands and extensive disease of the tongue through that incision. I am not particularly favourable to it. It seems to me that in cases in which there is such extensive disease as to warrant an operation like that, we had better not interfere with the neck, but content ourselves with removing the tongue.

You saw me in the beginning of last session do an extensive operation on a man in the private ward, No. 1. He was sent up from the country with extensive disease of the tongue and of the submaxillary tissues. He was in a miserable state, and anxious to have something done. I told him plainly that I would operate if he wished, but I could not promise him anything more than temporary relief. On November 2nd I operated on him. The disease was so extensive that I thought it better to do tracheotomy first. I do not usually do tracheotomy in these cases; I do not hold with preliminary tracheotomy in ordinary operations on the tongue; but this was so extensive a case that I thought it right to do it. I was then able quietly to remove the whole tongue down to the hyoid bone, and a piece of jaw right up to the masseter on each side, and all the infiltrated submaxillary tissues. It is not the first time that I have done an extensive operation like that. The first case I had was in 1875, and the result of it is in the museum. The patient, a man, aged sixty-two, was sent to me by a friend, with very extensive cancerous disease of the tongue and sublingual tissues. In January 1874, he noticed a swelling of one of the submaxillary glands, and soon after a sore beneath the tongue. The sore healed, and the gland subsided under treatment. In September 1874, the gland began again to swell, and at last broke. At the same time, he found that he had difficulty in articulating, as the tongue was fixed to the floor of the mouth, and, eventually, the tip of the tongue became fixed to the jaw. This last was temporarily relieved by an operation at another hospital. On admission to University College Hospital, the patient was unable to protrude his tongue or move it in his mouth; the saliva constantly trickled away, and articulation was very imperfect. He complained of great pain in the occipital region, but of none in the tongue. The gums of the incisor region were swollen and ulcerated, and the teeth loose. The tongue was fixed to the back of the incisive portion of the jaw, which was softened. All the tissues beneath the tongue were indurated, but the skin was not involved.

On September 29th, 1875, I removed the tongue, the centre of the jaw, and all the sublingual tissues, by dividing the skin in the middle line, then sawing the jaw through on each side, and, having isolated the tongue somewhat on each side, enclosed the whole of the disease with the wire of the galvanic écraseur. The parts removed consisted of the middle three inches of the lower jaw, nearly the whole of the tongue, and the sublingual muscles and glands *en masse*. At the posterior end, the mass measured two and a half inches in depth, and slightly more from side to side. The tongue appeared to be healthy, except at the anterior part; and on the left side, just behind the tip, was a nodule of the size of a pea. Beneath the tongue was a mass of a yellowish white firm tissue, with a granular surface, which was continued quite up to the cut margin. This tissue, on microscopical examination, proved to be epithelioma.

The patient made a good recovery, and was alive and well in 1890, *fifteen* years after the operation. He recovered a surprising amount of power of deglutition and speech, due principally to the growth of the stump of the tongue already noticed. The occipital pain complained of by the patient is difficult of explanation, but was entirely relieved by the operation. I have noticed it in other cases of cancer of the tongue (*Lancet*, 1876, vol. i.).

Still, these extensive operations are not very satisfactory. This last patient, before he was really well, had a recurrence. The operation was on November 2nd, and he went out on December 2nd convalescent from the operation, but having already evidences of a return of the disease. I heard from the friends that he had a recurrence, but I have not heard whether he is dead; the poor fellow cannot have long to live.

The satisfactory cases are those where the disease is entirely confined to one part of the tongue, where you can get well behind that portion and remove it at once, and where there is no infiltration of the glands. It rests with you, who will see these cases early, to insist upon the patients having an operation in early days if there is evidence of disease. When we get that sufficiently insisted on, unquestionably our rate of mortality and our rate of cure will very much improve.

The Registrar-General has pointed out that cancer is much on the increase. There are two or three explanations. No doubt many cases are put down as cancer which formerly were not recognised as such. Still, even if that be so, I think, from my own observation, there can be no doubt that we do see more cases of cancer of the tongue than we did twenty or thirty years ago. One explanation is, I believe, the greater spread of smoking; and another, I think, is the greater spread of syphilis. Those are the two main elements which have to do with cancer of the tongue being so much on the increase. I hope that we shall be able to diminish it, and that by taking the cases early, and by removing the diseased part thoroughly, we may be able to hold out some prospect to the patient of prolongation of life—we cannot honestly do much more than that at present.

ON

RETENTION OF URINE.

I PURPOSE to-day to speak of retention of urine, and it will be convenient to consider it at different ages. In the case of a new-born child there is sometimes a difficulty in passing water, which always depends upon a contracted orifice of the prepuce. When the child tries to make water it "balloons" its foreskin, swells it out, that is, into something like a child's balloon. Of course, the treatment in such a case is obvious and simple. You have only to dilate the orifice of the prepuce with a pair of forceps, or, if it is at all a severe case, you had perhaps better circumcise, and there is an end of the matter. But you sometimes meet with retention in the case of children who are a little older, and I believe that retention in children, not dependent upon a tight foreskin, is almost always the result of calculus in the urethra. A child may have a small calculus in the kidney without any obvious symptoms; he has never made any complaint, until one day you are sent for because he does not pass his water; perhaps for some hours the bladder has been distended, and there is obvious retention of urine.

Now in such a case always suspect a stone, and look for it; usually it is easily found just within the meatus urinarius. You take a silver probe and pass it down the urethra, and you feel it click against the stone, and if the calculus is just within the meatus it is an exceedingly simple matter. You have only to get it out with a little scoop, or if you have not one, the loop of a hairpin is a convenient thing. If the orifice of the urethra is at all tight, you had better nick it in order to get the stone out; but remember when you are doing this that you should always cut downwards, and not upwards into the glans penis. But sometimes the stone is not at the anterior part of the urethra, but behind; and these are more difficult cases, and are apt to be overlooked. The stone does not produce complete retention perhaps, although it makes a difficulty, and then ulceration is sometimes set up, with abscess, and possibly extravasation of urine; but where there is real retention, of course you can make it out by passing a probe or catheter down and touching the stone, and you may be able to feel it through the perinæum. In that case it is best, if you can, to extract the stone, but it is not easy to do it. You will have to put the patient under chloroform, and with a very slender urethral forceps you may be able to extract the calculus. More frequently the stone has become fixed, and you cannot get it out. Then the best thing is to cut upon it at once, for it is not a serious thing in children to open the urethra. You have the stone to guide you, and it is much better that you should at once cut upon it and take the stone out than leave it there to set up, as it certainly will do, irritation, and possibly, as I have said, abscess and extravasation of urine. Besides, if it is a case of *bonâ fide* retention, the child cannot be left many hours without being relieved.

Then there is one other thing in the case of children that should always be borne in mind—that the child may have tied a string or thread round the penis. Such cases are met with every now and then. Two or three years ago a child was brought one morning to my consulting-room with the glans penis hanging merely by a thread of cellular tissue. It was as nearly cut off as it could be, but not absolutely. I simply took a pair of scissors, cut the remaining thread, and took it off. Round the glans penis just where it had been divided there was a long hair; but whether that hair had cut it through, I cannot tell. The child utterly denied ever having had a thread tied round it, but it is just conceivable that a long hair might have been twisted round accidentally, and have cut its way through.

Whenever a child has a swollen penis which is not clearly a case of paraphimosis, you should ascertain whether there is a thread deeply embedded in the groove, which is formed by the swollen tissues all round.

Now let me pass on to a much more important matter, the retention of urine in adult age. With young adults, I suppose the most common cause of retention is some mischief supervening upon gonorrhœa. People who have perfectly healthy urethras do not get retention of urine. It is only after they have had gonorrhœa or some mischief about the urethra that they are liable to attacks of this sort. Sometimes you find a patient with retention of urine during gonorrhœa, or at all events in the later stage of it; and if that is the case, it is more usually due to some inflammation travelling to the prostate. The prostate becomes swollen and tender; if you put your finger into the rectum, you will feel it there; and it is because of that swelling of the prostate that the man cannot pass water. The obvious thing in such a case is to pass a catheter for him very gently through the prostate, and draw the water off. You proceed to treat the prostatic inflammation by a leech or two to the perinæum, and then the swelling generally subsides, but it may, of course, run on to an abscess; and remember that these cases of acute prostatitis in young men are more apt to run on into abscess than any other form of prostatic disease.

The later stage, after the gonorrhœa, is perhaps the more critical time for the urethra as regards retention. What happens is this. The man during the gonorrhœa has been debarred from all liquor, and then, assuming that he is well, he perhaps takes more than he should do. In hospital practice and amongst the lower classes you will meet with that kind of case much more frequently than in private practice. The better class of people do not, as a rule, drink more than is good for them; they do not indulge in that sort of debauch, but a labouring man is apt to take more liquor than is good for him after abstaining for a few weeks, and then he gets retention of urine.

That form of retention is by some surgeons thought to be purely spasmodic, others call it inflammatory. I believe it is really a little of both. If it is not inflammatory, it is at least congestive, and there is a certain amount of spasm mixed with it. Therefore, whether we call it congestive retention or spasmodic retention, we mean the same thing. At all events, clinically, you find a patient who has had gonorrhœa, and apparently recovered from it, who is unable to pass his urine. Sometimes such patients go on for many hours before they knock up the house-surgeon, so that at one or two o'clock in the morning you are called upon to relieve such a patient, and, of course, it is important that relief should be given at once. Now it is quite true that these cases will mostly get well of themselves, if you can have time. I mean if you give a patient of that class a good dose of opium, put him into a warm bed, and put fomentations about his perinæum, in the majority of cases the retention will pass off, and the patient will make water. But you must bear in mind the circumstances of the case. If it is a hospital patient, the house-surgeon is called up in the night, and naturally he wants to get to bed again. There are no great conveniences for putting the patient into a hot bath, or even into a hot bed, in the middle of the night, and so you have to pass a catheter for him; and, as Sir Henry Thompson says in one of his lectures, no doubt the patient much more appreciates your skill, if you pass a catheter without hurting him, and draw his urine off, than if you merely gave him a dose of medicine, put him into a warm bath, and let nature do the rest.

It is better, then, on the whole, to draw off the urine at once. The great thing is to use a good-sized catheter. These patients have no stricture, and you want a good-sized catheter; and of all catheters, commend me to the old-fashioned English gum-elastic catheter. If you take a catheter of the size I hold in my hand (No. 9), with a good blunt end to it, and if you warm it and oil it well, and pass it, with a stilette if you like or without, until you get to the obstruction point, you will find it generally somewhere about the bulb of the urethra; and then, if with a little gentle pressure you push against it, you will be conscious of a sort of yielding; and so, as you steadily push on, the catheter goes into the bladder, and you draw the water off. That, of course, is an extremely satisfactory thing for all parties; but the affair does not always come off so smoothly as that. You may find a difficulty in the way. Possibly there may be more congestion than you think, more spasm perhaps, and the obstruction does not yield quite so readily. Now the greatest mistake that you can make, I believe, is to use very small catheters. No. 7 is a very good average size, and, if you cannot get that in, the patient having no history of previous stricture, you had better put the patient under chloroform, and you will be able to pass the catheter with the greatest ease. It is much better for the patient that you should stick to that catheter or thereabouts, than that you should poke holes in his urethra with a small instrument; and if you give the patient chloroform, you overcome the difficulty at once.

I am aware that, when you get into practice in the country, you may have to modify your proceeding. When a medical man is on his rounds and is stopped somewhere because a patient has retention of urine, he may not have chloroform with him, and perhaps not a catheter. Those are the cases in which you may fall back upon opium, with warm fomentations. If you give a patient of that kind a good dose of opium, and have fomentations put about the perinæum, the probability is that in a couple of hours he will be able to make water. If he does not, then of course he will send for you, and you must be prepared to pass a catheter and draw the water off. I want you to understand that the case may be treated in two ways, and that it is really a matter of convenience which method you should adopt.

These are cases of spasmodic or congestive retention; it is a much more serious matter when you have a patient who has a tight organic stricture. The patient has perhaps had a stricture for years past, and he has been treated two or three times by different surgeons. People get very callous and careless about their urethras, and neglect themselves, and in that way the stricture gets tighter and tighter until at last some day, or more probably some night, the patient finds that he cannot make water at all, and he then sends for you to give him relief. These are confessedly very difficult cases to treat. You do not know anything previously about the man, you do not know the condition of the urethra, you have merely his story to guide you; do not, therefore, be deceived by what he tells you; do not begin at once with very small catheters, but always with a fair average size. For that purpose I do not know that you can do better than use the French instruments, the olivary catheters, beginning with a medium-sized catheter just to find out how the land lies. You pass that steadily down the urethra until it is stopped; then you carefully make a little pressure and try and get it through. If you find that it is of no use you abandon the attempt, and you steadily try one instrument after another until you get to the very smallest.

With care and skill—it requires a considerable amount of skill—you may insinuate through the stricture such a very small catheter as this which I hold in my hand, a catheter equal to about No. 0 on the English scale (it is No. 1 on the French scale), the smallest catheter I think in use. You may not succeed, and then you must have recourse to silver instruments. I know that silver instruments are not in fashion at the present time. Some surgeons hardly ever use them, but I think you ought to be able to use silver instruments, and that you may use them quite as safely as the soft ones. I presume, therefore, that having failed with these soft instruments, you try a silver one, Nos. 3, 2, or 1, or No. 0 if you have it, and do your very best to get it in. And let me say, without wishing to urge any violence whatever, that you must bear in mind that in these hard strictures you must use a little steady pressure, but no force, and you must be careful that the catheter does not suddenly slip from your fingers, and make a long false passage.

Then having tried all you know in the way of catheters, you put the patient under chloroform, and for this reason: in all these cases of retention there is a certain amount of spasm; and if you overcome that spasm by chloroform, you get the catheter in more easily than you can without it. Of course there is the mechanical obstruction, but there is a certain amount of spasm with it. When the patient is under chloroform he is necessarily recumbent, and then you have the advantage of putting your finger into the rectum, and seeing that your catheter is not in a false passage; you can feel it going in the proper direction, and perhaps assist it a little in passing in.

In a case of that kind, when everything has been done in the way of catheterising that is possible, and yet the patient is not relieved, the bladder is nearly or half-way up to the umbilicus, and the patient is in considerable distress. It is clear that the patient cannot be left, because, if he is, there will be a risk of the urethra giving way at some point, and possibly of the bladder giving way, but that is, of course, an extremely rare occurrence. Therefore the problem resolves itself into what is the safest thing to be done in a case of retention of urine with an impassable stricture. I have no hesitation in saying that the safest thing is to aspirate the bladder above the pubes, and you cannot well go wrong if the aspirator is in proper order, with a fine needle, which you take every precaution to make aseptic, carefully carbolising the whole machinery, and washing the skin of the abdomen with carbolic acid, then puncturing a little way above the pubes. That operation seems to be attended with as little risk as possible. Yet, a year or two ago there was a paper read at one of the medical societies by an eminent surgeon, in which he endeavoured to show that this operation was not free from risk. The point was this, that if you let out putrid urine in that way, it was possible that you might get some of the urine into the cellular tissue about the pelvis, and mischief might occur. I think he was quite right; it is quite conceivable; but fortunately in these cases of retention, the urine is not putrid; it is usually a case in which the urine is fairly healthy, and, therefore, you may hope that, in the majority of cases, you will not run much risk in aspirating the bladder above the pubes. At all events, I recommend you to do that rather than undertake any serious operation.

But, naturally, aspiration is only a temporary arrangement. You would not think of keeping the aspirator needle in. You merely draw off the urine, and then hope that the difficulty may be overcome in other ways. When you have aspirated once or twice, it might be better to puncture above the pubes with an ordinary trocar and cannula somewhat curved, leaving the cannula in (there is a cannula made on purpose). Patients may no doubt go on for years making water in that way through a permanent opening above the pubes. On the other hand, if you do not choose to adopt that method, there is the much older plan of puncturing through the rectum. There are disadvantages in that, and I think that surgeons generally have given it up. It is still used at Guy's Hospital, I believe; because Mr. Cock, formerly surgeon there, specially advocated it, but I do not think that the method is often adopted elsewhere. The objection to puncturing by the rectum is this. In the first place, it is inconvenient to keep a tube in the rectum, and in the next place there is a risk of wounding the peritoneum. The peritoneum is not always so high as it ought to be. Sometimes it comes lower down to the prostate than you would expect, and, under those circumstances, if you puncture it, you are sure to get peritonitis. Then, again, it is possible that a little urine may get out, and thus you may get cellulitis about the pelvis. So that there are several drawbacks, and altogether the operation is, I think, practically superseded by the other proceeding.

If a man has a tight stricture in the perinæum, which is impassable, and if he has also retention of urine, of course it becomes a question whether you would not be wise, under some circumstances, to operate in the perinæum and divide the stricture, and, at the same time, relieve the retention. I quite allow that, under favourable circumstances, that is, perhaps, the best proceeding. I mean, of course, where you have plenty of assistance, plenty of good light, and every convenience for an operation, such as we have in hospital practice. In such a case an operation in the perinæum is perfectly justifiable, and is sometimes better than anything else. But under the circumstances that many of you will have to deal with, where the patient is entirely isolated, and where you have no conveniences and no assistance, I should strongly advise you not to attempt any perinæal operation. It is always a difficult proceeding, and sometimes it is an extremely arduous operation to perform.

Now, if you elect to do any perinæal operation in a case of retention of urine, I should strongly recommend you to do Wheelhouse's perinæal section, which is, in fact, the only operation that would be performed at the present day. Let me point out shortly what the operation is before I go on to other matters. You take a straight staff, like that which I hold in my hand, with a little knob at the end, and you pass it down to the obstruction, which is, of course, usually about the bulb. The patient is then tied up in the lithotomy position, and the surgeon opens the urethra quite in front of the stricture, presumably a healthy portion of the urethra. Having opened it upon the groove, the assistant turns the instrument round, and hooks it up so as to draw up the urethra a little, and with two pairs of forceps the urethra is held open on each side. Having exposed the interior of the urethra, the operator has to find the orifice, which is, of course, exceedingly small. He takes a probe-pointed director, and, having passed the probe through the stricture, he pushes the director portion a little on into it, and with a bistoury he divides it. Then, still keeping the groove there, he passes a gorget into the stricture and along the urethra, and so guides the catheter through the whole length of the urethra, and in that way into the bladder.

And now I come to the last type of retention, at the other extremity of life—in old age; and let me say that this is a class of case which you are likely to meet with in private practice. Old people get retention of urine, not because they have enlarged prostates merely, for many men have enlarged prostates after sixty, and plenty of them go through life afterwards without having retention of urine. The reason why they have retention is because they get congestion of a large prostate, arising generally from exposure to cold. What happens is this: an elderly gentleman, who has been wearing thick clothes all the day, dresses in the evening to go out to dinner, and puts on a very much thinner pair of trousers. Without the slightest reflection upon his appetite in any sense whatever, he is exposed to cold and gets congestion of the prostate. It is very uncharitable to say that all these elderly people have taken more wine than is good for them, or anything of the kind; it is not the fact. Perfectly reasonable and sober persons get retention simply from exposure to cold. They find, when they go to bed, that they are not able to make water; they go on trying, perhaps, to make water at intervals during the night, and late at night or early in the morning you are sent for, to find an elderly gentleman in considerable distress because he cannot relieve his bladder. Such a case as that of course requires a little careful management; but, after all, it is not a very desperate case to treat, unless, unfortunately, the old gentleman has been trying to pass a catheter for himself. The cases which give trouble are where you find that the patient has been making false passages with his own catheter. Some time or other, probably, he has been provided with a catheter, and then, getting retention of urine, he tries to use it, and he makes himself bleed. In such a case you may have a good deal of trouble; but, fortunately, in the great majority of cases no catheter has been used when you are first called in. In going to a case of that kind, of course you provide yourself with suitable instruments. You want an ordinary English catheter of good size, No. 9 or 10. A soft india-rubber catheter is also a very useful thing; and then there are the elbowed or *coudé* French catheters, such as the one you see here. Usually, with one or other of those instruments you can overcome the difficulty if you only go to work quietly and steadily, giving your catheter a good curve, and slipping it down until you come to some obstruction, then gently pressing against it till the catheter goes in. I think that the old-fashioned English catheter without a wire is the best instrument, but the fashion rather runs on these *coudés*.

Occasionally there are great difficulties. The third lobe of the prostate rises up in the floor of the urethra, and makes it exceedingly difficult to get into the bladder, and in these cases you may sometimes advantageously push the catheter in as far as it will go with the wire in it, and then withdraw the wire and push the catheter on a little further, and, softened as it is by the heat of the urethra, it finds its way in. Of course you must bear in mind that the projection of the prostate may be on one side, and that is one great reason why you cannot put the catheter straight in; but you must let it fit itself as it were to the urethra, and gradually find its way into the bladder. However, with care I believe that all these cases are to be successfully treated. You may be obliged to have recourse to a silver instrument; it is better not to do so if you can help it, but anything is better than leaving the patient unrelieved; and the large, well-curved silver catheter is a very useful instrument in such cases. The size I prefer is about No. 10, and you want a good long curved catheter in these cases. Sometimes where the other catheters have been tried and have failed you can pass a catheter like that without any difficulty, if you will only keep along the roof of the urethra.

Now, in such a case of retention, supposing that you have drawn the water, what kind of urine will you expect to see coming through the catheter? It is always more or less bloody from the congestion of the prostate. Patients are apt to be alarmed by this, but you need not be, for it is just one of the ways by which nature relieves the congestion of the parts, and the urine will be bloody for some few days, and then gradually will clear up, and the patient will recover his usual habits. It is also well to bear in mind that these patients do not at once recover the use of the bladder; they cannot micturate for some days. The time varies in different cases, but it is not at all uncommon to find a patient who has had a bout of prostatic retention, going on for a fortnight or three weeks before he can thoroughly make water for himself. It is always well to know that, so as to be able to warn the patient's friends that you will have to come and draw the water for him for a few days until he recovers entirely the power over the bladder.

I have now given you all the varieties of retention in the male, from the baby to the old man. Let me say a word about retention in the female, which occurs from time to time as the result of some displacement of the uterus—the tilting backwards of the unimpregnated uterus, or possibly of the impregnated uterus, or may result from the natural changes of pregnancy. In the later weeks or days of pregnancy, when the uterus is pressing considerably upon the neck of the bladder, one can easily understand how it produces retention of urine. I simply mention it here because it is just one of those things that are apt to be overlooked. A woman makes a certain amount of water, but the bladder is more or less distended, and therefore, when you are called in to attend a labour, always ascertain whether the bladder is empty or not. I suppose there are no more unfortunate cases than those where a child's head has been allowed to press upon a full bladder, for those are the miserable cases where you have ulceration and consequent vesico-vaginal fistula. If you always pass a catheter or see that the bladder is empty in every case of labour, you will hardly ever have such a thing as vesico-vaginal fistula.

There is another class of cases of female retention which should not be treated with the catheter; that is the hysterical. Young women sometimes cannot or will not micturate, and it is a mistake to use the catheter if you can avoid it. Every now and then you are obliged to use the catheter, but it should not be used if it can be avoided, because you can really treat those cases more satisfactorily without it. The great thing is to put the treatment in the hands of a judicious matron. Let her put the patient into a bath of warm water, and then, without giving her notice, throw a bucket of cold water over her back and shoulders. The sudden shock from the cold water over the trunk will produce a violent expiratory effort, which will overcome the already relaxed muscular fibres of the sphincter vesicæ, so that the urine passes, and there is an end of the difficulty.

ON THE

COMMON DISEASES OF THE BREAST.

To-day I will speak of the common diseases of the breast. Some of them you may see here in the wards of the hospital, and others you will have opportunities of seeing, I have no doubt, in the out-patient department, and you may also meet with them in private practice.

First, let me say a word with regard to atrophy of the breast. You will, of course, find different individuals with their breasts differently developed; for in some families there is hardly any development of the mammary region, while in others you find that all the girls are full-breasted. The practical surgical point is whether or not the breast is sufficiently developed, particularly the nipple, for the duties of maternity. That is where the atrophy of the breast, if we may so call it, becomes important in practice. If you are retained to attend a woman in her first confinement, it is quite worth while to take a little trouble before she is delivered to see that she is able to nurse the child which will be born. Many women have very sessile nipples; and you must remember that the nipple is not merely a series of ducts for the passage of the milk, but that there is a large quantity of involuntary muscular fibre in it, and it is because of that involuntary muscular fibre that the nipple, when it is excited, becomes more prominent, thus enabling the child's mouth to retain it. You may notice that when a child is first put to the breast it will not be able to grasp the nipple, but, by poking its nose and mouth against it, the nipple is excited, so that it becomes more prominent, and then the child can hold it.

It is quite worth while to draw out the nipple, if it is too sessile for the child to get at it, by an ordinary breast-pump. The old-fashioned breast-pump, I suppose, has quite gone out of use. The more modern instrument is one with which a woman can draw the breast herself, and may be seen in all the chemists' shops. It consists of a little glass shield, with a mouthpiece coming off from it, at which the woman can suck, so making a vacuum; and the milk then comes away and collects in the lower part. But a little friction also may do good in developing the nipple—a little gentle rubbing, too, will harden the skin, and that is an important matter. But let me say that if friction is carried on to an abnormal extent, it may produce hypertrophy of the breast, which is a serious thing, leading to amputation.

You will find that the monthly nurse, if she is a good one, will take a little trouble to bathe the nipple of a primipara with a little brandy-and-water, which is a common recipe, and a very good one; but one great reason why nipples become sore is that they are so constantly wet. You may not be aware that many women suffer from an involuntary flow of milk from the breast, so that their clothes are apt to get wet, and I have here a little contrivance to be used in cases of that kind. It is a flat glass, into which the nipple can be slipped, and it can be worn under the dress. The milk which is superfluous accumulates in the lower part of the glass, and does not wet the breast. It is a great point that the nipple should be dry, and that it should be protected when it is not being used; otherwise it is apt to become what is commonly called "chapped." Now, a chapped nipple is the beginning of a series of troubles, and it should therefore be cured as soon as possible. I have asked in the midwifery department of the hospital what is now the favourite remedy for chapped and sore nipples, and I am told that it is glycerine of tannin and sulphurous acid, of each half an ounce, with one ounce of water. You have there the astringent effect of tannin and sulphurous acid, and I have no doubt that it is a good remedy. There are fifty remedies for chapped and sore nipples; and perhaps one of the best surgical ones is nitrate of silver, if properly applied. The great mistake made in the application of nitrate of silver for a sore breast is in taking a stick and rubbing it on promiscuously; you then have a great patch of caustic, and you may have a large sore on the nipple. The proper way is to take a fine camel-hair brush and wet it with a ten-grain solution, or draw the brush over the stick of nitrate of silver, and, having thoroughly dried the nipple, to paint into any little crack that you can see.

Some sore nipples evidently depend upon the condition of the child's mouth. A child with an aphthous mouth is apt to produce a sore condition of the nipple, and no doubt it may be communicated from one child to another; for if a healthy child is put to a nipple of that kind, it may possibly get thrush. Borax is the great remedy for the complaint—the old-fashioned borax and honey, or the more modern glycerinum boracis, applied freely to the child's mouth, and also to the nipple of the mother.

These are comparatively simple cases; the complicated and important one is when syphilis is present in the child's mouth. When you get into practice you cannot be too much on your guard about syphilis, that is to say, you must keep your eyes open but your mouth shut. If you bear it in mind, you will avoid the errors into which practitioners are sometimes led, both as regards the mother and as regards the wet-nurse, which is an important point. A woman, we will say, has borne a healthy child, and she is unable to nurse it. A wet-nurse is engaged, and you find some time afterwards that the child develops a coppery rash upon the body, with all the symptoms of constitutional syphilis, and it becomes clear that the child has been syphilised by the nurse. Such cases have occurred from time to time, and they are extremely serious. It is very important that in selecting a wet-nurse you should go thoroughly into the history of her previous life, and inquire whether she has suffered in any way from sores about the genitals, or whether she has had secondary, or, possibly, tertiary symptoms. Generally the wet-nurse is young, and will not be likely to have got so far as tertiary symptoms; but it is not uncommon for a young woman to have unconsciously secondary syphilis from a husband, or in other ways, because wet-nurses are not always married. You find, perhaps, that there is some evidence of syphilis in the throat, and she may communicate this to the child she suckles by kissing it.

I do not think there is any proof that syphilis can be communicated through the milk, but what more often happens is that the woman's nipple becomes a little sore, then you get the child's mouth infected, and all the symptoms of constitutional syphilis develop; and it has sometimes happened in such cases that the syphilised child has been put to the nipple of a healthy woman, and has again communicated syphilis. You cannot be too particular in the choice of a wet-nurse, and you should be most particular in looking at the child that you give to the wet-nurse. If you have delivered a woman who is unable to nurse her child, and who is the subject of syphilis, the child will undoubtedly within a month or six weeks after birth develop congenital syphilis; and if you have already put the child to a wet-nurse, the chances are that it will communicate syphilis to her, and then trouble may arise—an action might be brought, or remuneration would have to be made. I draw your attention to these things because mistakes are sometimes made simply through inattention to them.

Now, supposing a patient has a simple ordinary sore nipple, what bad results may result from it? The first thing is that the unfortunate woman with a sore nipple is almost unable to nurse her child. The nipple is so exquisitely tender that when the child is put to it the woman, whether the mother or not, cannot bear the pain. Under those circumstances, of course you have to do something, and the best thing you can do is to provide an artificial nipple. A glass shield, like that which I hold in my hand, is the usual thing. Upon it there should be fitted an ordinary india-rubber teat, and if that is placed over the nipple, and the nipple is made to go well into it, with a little suction by the child's mouth the milk flows, and there is no irritation of the nipple. You can then proceed with the medication of the nipple, without doing harm to the child.

But it often happens that the woman neglects herself. She has got a sore nipple, she cannot nurse the child, and she allows her breast to become gorged with milk, which is the first stage of inflammation of the breast. You will sometimes have a woman come to you with a breast as full of milk as the unfortunate overstocked cows that are often driven to the fair, not having been milked for twenty-four hours before selling, in order to show the purchaser that they have good udders of milk. The woman suffers in the same way as the lower animal; the distension of the breast is extreme, there is great weight and distress, and, if it is not shortly relieved, it soon ends in inflammation. The mode of relieving it is a simple matter. The milk may either be drawn with a pump, or the breast may be relieved very much by gentle pressure of the hands, the operator standing behind the patient and compressing the breast, so that the milk is squeezed out without difficulty. I say gentle pressure, because if you use anything like violent pressure, you may do a great deal of harm. In the case of new-born children, one sometimes sees a foolish old monthly nurse who observes a little fluid coming away from the breast of a child (which is not at all uncommon), set to work vigorously to squeeze it out, and every now and then you get in such cases an abscess in the breast of a new-born child. If, as I have said, the woman's breast is gently squeezed, the milk exudes, and the trouble may pass off.

If it is necessary to stop the milk in the breast—if, for instance, the child has died, or has been taken away from the mother, and the breast becomes overloaded, then there is nothing so good as the application of belladonna. At the present time in this hospital, and, I think, a good deal through my efforts and teaching, we use belladonna for most local inflammatory ailments; but originally the use of belladonna was introduced for breasts in which we wanted to arrest the secretion of milk. There is no doubt that the application of belladonna mixed with glycerine is the very best remedy that you can have for that kind of case. Well and thoroughly applied over the breast, it tends to arrest the secretion of milk, relieves pain, and prevents the occurrence of inflammation. Then suppose we have the breast not properly emptied, it having gone beyond that stage before you see it, you certainly will have inflammation set up in the breast—general mastitis, if you like to use the word. There are two theories with regard to that inflammation. I believe myself that in the great majority of cases it begins, as I have described, in distension of the milk ducts; but some authorities believe that the inflammation travels up the ducts from the sore nipple, and they say that the nipple is the direct cause of the inflammation. Be that as it may, you find all the symptoms of inflammation—namely, swelling, heat, and redness—and the great thing, of course, is to relieve it as soon as possible.

If there is one thing that a woman, who has a sore breast, objects to more than another it is to having it, as she calls it, "lanced." If you say to a patient that you are going to lance her abscess, she will probably button up her dress and disappear, and you will not see her again. When I used to see the out-patients I generally got over the difficulty in cases of this sort by avoiding the word "lancing," and by making no open show of knives and basins. If you open your lancet behind your back or in your pocket, and then just make a puncture, which is really all that is required, you can let out the abscess without any great difficulty, and your patient will be only too grateful for what you have done. Of course, if you are going to open an extensive abscess, it is wiser to give the patient chloroform; but if you have not the opportunity of giving chloroform, you must act artfully and open the abscess quite sufficiently without giving the patient any pain; the puncture is so momentary and the relief of tension of the abscess is so great that the operation is really painless.

With regard to the opening of an abscess of the breast, in what direction should you open it? No doubt the best plan is to open in the direction of the ducts as far as possible. You know, I presume, that from the nipple there are straight ducts radiating, and then the lobuli of the breast are arranged all round, so that in making the incision anywhere near the nipple you should always make it in a line radiating from the nipple, and, as far as possible, in a dependent position. Of course, if the abscess happens to be in the upper part of the breast, an incision should be made immediately above the nipple, that is as good a position as you can have; but if the abscess is in the lower part of the breast, clearly you must make it lower down. You must make a dependent opening wherever it is, and you must remember the anatomy of the gland so as not to cut the essential parts of the breast across more than you can help. In the wards, from time to time, we see cases of abscesses of the breast, and they are invariably the neglected ones, for we do not ordinarily take in cases of common abscess of the breast. Mr. Holder has given me the notes of three cases which we have had in the wards this year. I will not trouble you with the details of two of them, which are very common cases, but will pass on to that of a woman named E. B., which I will give a little in detail.

She was aged twenty-one, and was admitted on

June 27th. She was confined with her first child (these abscesses generally occur with the first child) on May 30th, 1888. She tried to suckle the child for two days, but had not enough milk.

On June 15th she had great pain in the right breast, and during the night the breast swelled to a great size.

On June 16th the breast was poulticed till the pain ceased. The left breast swelled in a similar way, and it was poulticed.

On June 17th the right breast began to discharge about the lower border, and very soon a number of discharging points formed, and a large amount of pus flowed during the following week. Within a few days the left breast began to discharge, but it was never so bad as the right one.

On June 26th the right breast ulcerated right through above the nipple. The lower part of the breast being thus detached fell downwards by its own weight, disclosing a large abscess-cavity between the breast and the pectoral muscle.

On admission the right breast was pretty well converted into a large abscess. At the top, two inches above the nipple, many discharging points joined together, forming a large, irregular, ragged wound of a linear character, chiefly transverse. The result was the falling of the breast in a flap downwards, exposing the whole cavity of the abscess. Those who saw the case can hardly forget it; the condition was so remarkable on account of the enormous destruction of tissue, and the large abscess, which was very widely open. The abscess was submammary, the floor being formed by the pectoralis major, the falling flap containing the whole thickness of the breast, and, except for a questionable portion below, its whole substance.

This is an exaggerated example of the bad effects of over-poulticing. I do not know whether the woman had medical advice or not, but she seems to have been advised to poultice, and that is just what is done by ignorant patients; they go on poulticing until they reduce the vitality of the part, and the whole of the tissues have become undermined by suppuration, and then at last the woman has to come into the hospital. Let me remind you of what was done before I show you the patient. The great point at first was to support the breast, and you may remember I insisted on its being properly supported with plaster. It is of no use bandaging in these cases, but with long strips of plaster properly applied you can make the pressure sufficient to bring the parts into position.

You now see from the appearance of the patient before you, that the description Mr. Holder has given is perfectly correct. There is a great transverse cut running across the breast, and the whole breast fell down. On the other side you see there are scars of a less severe suppuration, a part of the breast was a good deal undermined, but it was nothing compared with the right side.

As regards the treatment of these mammary abscesses, in the first place let me say that I do not decry poultices. You must have heard me say in the wards that I have not lost my faith in the old-fashioned linseed poultice, and I believe it to be an extremely comfortable thing if properly applied, and if the breast is properly supported. The great mistake is keeping it on too long. A bad breast should be held up either with a silk handkerchief or a bandage. If it is kept up and well supported, I have no objection to poulticing for a few hours, but as soon as there is any matter present it ought to be let out. That is the difficulty—that the matter is not always at the surface, and the surgeon hesitates because his finger is not sufficiently educated to be sure whether there is any matter or not, and he allows it to burrow; whereas if he were more up to his work, he would have made a timely incision, and saved all the trouble. For myself I would much rather make an incision uselessly in a case of that kind, before the matter is formed, than I would allow the matter to burrow, even twelve hours after it was formed. You do no harm by an early incision, for if nothing escapes you withdraw the knife, and the puncture soon heals; but if you find the matter deeper than you expect, and it wells up beside the bistoury, you have only to enlarge the opening, and the matter flows out.

If the case comes before you after the matter has burrowed, a great deal can be done by the modern system of putting in drainage-tubes, which in these cases are extremely useful. You put a drainage-tube properly arranged, so that the opening shall be dependent, and the matter finds its way out, and you relieve the patient materially. Here, again, let me warn you against putting in too many drainage-tubes and keeping them in too long. There is sometimes a tendency, when a drainage-tube is put in, to leave it in indefinitely, whereas it does all the good it is likely to do in two or three days. You will notice how soon we remove drainage-tubes here in cases of amputation and the like. It is of no use to leave them in a long time; in fact, they become practically setons.

Then let me give you another caution. Be careful that you do not lose your drainage-tube. I have more than once seen cases in which a breast has been drained, I have no doubt very properly, but unfortunately the surgeon has lost a piece of the drainage-tube. Months afterwards the patient in such a case comes back, and there is a great hubbub because the piece of drainage-tube has come out of a sinus, and naturally the patient appreciates the fact that the trouble has been kept up by the tube. You should, therefore, have strings to your drainage-tube, or you may adopt the plan you have seen me use of putting a safety-pin through the end of it, which, I think, is better, because it always keeps the end of the tube flush with the skin.

So much for the ordinary form of abscess. Let me remind you that you may have mammary abscesses in different situations. There are three common situations in which an abscess may form. First, you may have it in the straight tubes round the nipple. One of these little tubes is blocked by epithelium; the tube then becomes distended with milk. If you see it, you may open it, and call it a little lacteal tumour if you like; but if it is not seen and treated, it is apt to suppurate, and then you get a little elongated abscess close to the nipple. The treatment is quite obvious and simple, and you merely incise it. A more serious case is the one I have described—an abscess in the substance of the breast. Then, lastly, there is the form which you meet with occasionally, but not commonly—an abscess under the breast. In the case the notes of which I have read to you, by the time the patient was admitted the abscess had got under the breast, but I do not think that it was so originally. But there are cases in which the cellular tissue between the breast and the pectoral muscle becomes inflamed, and the abscess forms there, and you have the remarkable feature of the breast being thrust forward by a swelling behind it; and the fluctuation, instead of being through the breast, is at the margin of it, and usually the lower margin. In these cases, of course, you would make a free incision below the breast and let the matter out. It runs out readily, and afterwards, by judicious pressure, you bring the breast back again into its original position, and it soon gets a firm attachment. Those are comparatively simple cases if they are recognised, but I have known an abscess of that kind to be taken for a much more serious disease. The patient was thought to have a rapidly growing tumour of the breast, which, after a time, resolved itself into a small abscess over the pectoral muscle.

Let me now say a word with regard to a simple matter, but one which is sometimes misunderstood from the want of having seen it. There is a disease of the skin which we know as molluscum contagiosum, which is found upon children's faces, and which you may sometimes find upon a woman's breast. It is a little collection of sebaceous material, and if examined microscopically will be found to consist of cells packed together in lobular masses, bounded and separated by fibrous tissue. Some authorities do not agree as to its being contagious, but I have no doubt it is contagious in this sense, that if a child happens to have it on its face—a common situation—you will find that the mother's breast will become inoculated, and there will be a development of molluscum contagiosum upon it. I have here a picture of it. You see the sebaceous material collected in the follicles of the child's face, and there is an exactly similar disease produced upon the mother's breast. I am not sure that it can be communicated from the mother to the child, but I am sure that it can be communicated from the child to the mother. It is not a serious thing, but you ought to be able to recognise it. The treatment is to squeeze out the little sebaceous collections, which heal up readily enough, as the ordinary comedones do on the face.

Then there is another thing which is occasionally found on the skin of the breast—that is keloid. It is not common, and I mention it for that reason. You may meet with a patient who has developed keloid on the skin of the breast, which is very apt to be mistaken for a more serious disease. It is hard, and at first sight looks a little like scirrhus. I simply mention it that you may have your attention called to its occasional occurrence.

ON

CASES OF SARCOMA.

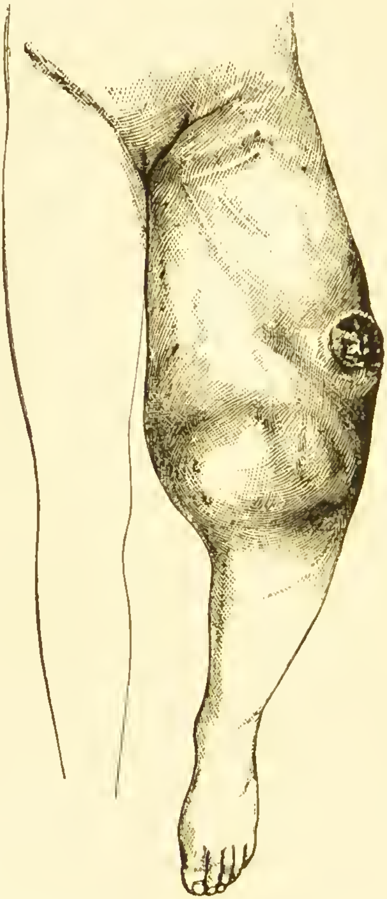
As I have had occasion recently to operate upon some remarkable cases of sarcoma, I take the opportunity of making a few remarks upon this form of disease. And, first, what is meant nowadays by sarcoma? Modern pathologists regard as examples of sarcoma tumours consisting of embryonic cells embedded in a matrix, but not contained in alveoli like the true cancers, the blood-vessels running between the cells themselves. This is all very well when the tumours are removed and submitted to microscopical examination, but it will be necessary for you to make a diagnosis, if possible, before employing the knife, in order to guide you as to the treatment to be employed.

Clinically, sarcomata differ very much from cancers, although some of them are little less malignant. They occur in young or middle-aged patients, and their rate of growth varies directly with their malignancy. Whilst more apt to develop cysts than cancers, the tendency of sarcomata to involve the lymphatic system is much smaller, except in the case of the melanotic sarcoma, in which the glands are early and extensively involved. The sarcomata affecting bones, of which we have recently had examples, may be best divided into periosteal and central, of which the periosteal is the more serious, both as regards rapidity of growth and infiltration of surrounding structures. The periosteal may be spindle-celled, round-celled, or ossifying; whilst the central may be spindle-celled, round-celled, or myeloid, which last is undoubtedly the least malignant of all. The articular ends of the long bones are the favourite seats of all these forms of sarcoma of bone, which affect also very commonly the bones of the face, and especially the jaws. The recognition of a periosteal tumour of bone is comparatively easy, as the growth can be made out with care almost as soon as the patient's attention is called to the part by the pain, which is usually a prominent symptom; and the way in which the periosteal tumour creeps up the shaft of the bone is very characteristic. The central sarcomata are less defined; but they have very commonly the peculiar crackling of thin bone over them, and not unfrequently present a very distinct pulsation. Early removal before the system has become infected by secondary deposits in internal organs is the only treatment possible in sarcoma. In the cases of sarcoma of bone the periosteal variety requires much freer removal than the endosteal, on account of its tendency to infiltrate surrounding tissues; hence amputation, if possible at or above the next joint, offers the only prospect of permanent relief.

The following is a summary of a case of tumour of the thigh, which I recently amputated at the hip-joint successfully, and I would especially direct your attention to the treatment of the patient immediately before, during, and after the operation, because I think the recovery of the girl depended a good deal upon it; and, without laying too much stress upon rapidity of operating, it is, I think, a mistake to run any risk of loss of life by undue deliberation and consequent exposure of the patient.

Case 1 (from notes by Mr. Pollard, surgical registrar).—E. M. J., a girl, aged sixteen, was admitted under my care on September 26th, 1887. She had enjoyed good health until six months previously, when a swelling was noticed in the middle of her left thigh. This swelling rapidly extended upwards and downwards. The patient was seen by myself nine weeks before admission, and amputation was advised, but the patient's friends preferred to try the powers of a herbalist, who treated the disease for about six weeks. During that time the growth increased very rapidly in size, and became very painful. For three weeks the patient had been confined to bed. On admission the whole of the left thigh was enormously enlarged (Fig. 2). The superficial veins stood out as blue cords, some of which

Fig. 2.



measured one-third of an inch across. The thigh was very tense, but elastic, and in some places distinct fluctuation could be obtained. At the upper limit of the growth, close to the groin, the circumference of the thigh measured 23 inches; at the middle and lower end of the growth it measured 25 inches. The corresponding measurements of the sound thigh were $10\frac{1}{2}$, $11\frac{1}{2}$, and 14 inches respectively. The patient evidently suffered most severely from pain, which was, however, almost completely controlled by an injection of four minims (0.4 gr.) of morphia night and morning. The weight of the limb was so great that the patient was unable to move it. Her temperature ranged between 100° and 103.2°. There was some healthy skin in front just below the groin, and the buttock was not implicated by the disease, so I determined to remove the growth by disarticulating at the hip-joint. This was done on the ninth day after admission.

For two hours before the operation the limb was supported at an angle of 110° with the body, so as to allow as much blood as possible to drain away from the limb; and half an hour before the operation four ounces of brandy were injected into the rectum. The circulation was controlled by a Signorini's tourniquet applied to the aorta.

Owing to the high extension of the growth on the anterior surface of the limb, it was only possible to make a short flap on that aspect. The anterior flap was cut from without inwards. It was composed of little more than skin and fat. The knife was rapidly sunk through the muscles, and the capsule opened. As the femoral vessels were divided and secured, a gush of blood took place, mainly from the distal side of the section. After the head of the bone had been disarticulated, and the muscles severed from the trochanter, a long and fleshy posterior flap was cut from within outwards. Notwithstanding the delay in order to secure vessels, and the inability to manipulate the thigh forcibly, owing to the femur being broken, the limb was detached in less than three minutes.

There was very little loss of blood. The vessels were secured with catgut ligatures; the flaps were adjusted with silver and catgut sutures. Two drainage-tubes were inserted between the flaps. The stump was dressed with iodoform powder and iodoform and salicylic wools. During the operation two ounces, and at its close four ounces, of brandy were injected into the rectum; a total of half a pint of brandy was given per rectum in two hours. As soon as the limb was removed, the remaining one was raised to an angle of 120° with the body, and kept in that position for eighteen hours. The head was not supported by a pillow, and the foot of the bed was propped up.

The patient showed no signs of consciousness for two hours and a half after the operation. The pulse was very rapid (120 to 140 per minute), but responded to frequent small doses of brandy. The dressing was changed five hours and a half after the amputation, as it was soiled with fæces. The inner half of the wound was found soiled. It was washed with carbolic acid lotion (1 in 30) and solution of perchloride of mercury (1 in 500), and re-dressed. The inner part of the dressing was sealed down with collodion. By this time the patient was sensible, quiet, and warm. The stump was dressed on the second, fifth, sixth, ninth, tenth, and twelfth days after the operation. The drainage-tubes were removed on the fifth day. The temperature rose to 103° on the second day, then sank below 101°, and reached the normal on the sixth day. It afterwards ranged between the normal and 100.4°. On the twelfth day a quantity of pus was found bagging in the flaps. The inner part of the wound had healed, but union at the outer part had failed. On the sixteenth day all the stitches were removed and the flaps were supported by strips of strapping. After the twelfth the wound was dressed daily. On the twenty-fifth day the patient got up, but the flaps were not then healed. She left the hospital on the forty-second day after the operation.

After the amputation the patient's condition was much improved. Before it she suffered severe pain, and was quite helpless in bed owing to the weight of the tumour; after it the pain soon subsided, and in less than a month the patient left her bed. On examination of the parts removed, it was found that the growth was entirely got rid of. The great size of the latter was due largely to the presence of three cysts, which together contained ten pints of blood-stained fluid. The growth was a spindle-celled sarcoma, which had commenced centrally in the shaft of the femur. In some places the tumour was covered with plates of bone, but over the greater part of its surface the bone had entirely disappeared. The upper and lower ends of the femur were surrounded by the new growth, and the bone was broken at about the junction of the middle with the upper and lower thirds.

The week following that in which I amputated at the hip you saw me amputate at the shoulder-joint of a man whose history was remarkable, inasmuch as he was unaware of anything wrong with his arm until the bone broke on a slight exertion. The following was his history.

Case 2.—J. R., a man, aged thirty-five, was admitted on October 8th, 1887. There was no family history of new growths, and his parents had reached old age. He was a carpenter, and had frequently been struck on the left shoulder by pieces of falling timber, but he had never sustained any severe injury of it. About five months ago he noticed some enlargement of the upper part of his right arm. He was able to continue his work, but with some difficulty, owing to pain, until the day of admission, when he raised his arm to strike one of his children. His arm then dropped to his side. The right shoulder and upper arm were much swollen, and manipulation of the limb produced crepitus and increased mobility about the insertion of the deltoid muscle. There were two well-marked swellings in connection with the upper part of the shaft of the humerus, which might have been mistaken for the two ends of the broken fragments overlapping one another to the extent of two inches, had it not been that there was no shortening of the total length of the limb. It appeared evident, therefore, that the swellings were due to the presence of a new growth, which would also afford an explanation of the occurrence of a fracture from slight muscular action only.

On the fourth day after admission I raised a deltoid flap and verified the diagnosis of new growth. I then disarticulated the head of the humerus and cut the inner flap from within outwards. The vessels were quickly secured. Iodoform powder was dusted into the wound, and the edges adjusted with sutures. One drainage-tube was inserted at the posterior angle of the wound, and a dressing of salicylic wool was bandaged on. During the operation an ounce of brandy was injected into the rectum. After the operation the patient was very collapsed. Ten minims of ether were injected beneath the skin, and two drachms of brandy were ordered every half-hour. On the day after the amputation the patient recovered from the shock; he was still weak, but felt fairly comfortable. On the second day the temperature reached 101°; it then fell, and on and after the seventh it was about normal. The dressing was changed every second or third day. The wound at first appeared to be healing without suppuration, and on the sixth day a very small piece of drainage-tube was employed. On the tenth suppuration had occurred, and the pus did not escape freely until a stitch was removed and a tube placed between the flaps. After the last date the progress towards recovery was continuous, and on the twenty-sixth day after the amputation the stump was nearly healed.

The growth involved the anterior and inner aspects of the humerus. Its upper limit was just below the neck of the bone, and its lower at the insertion of the deltoid muscle. There was an oblique fracture of the humerus, which started at the inner side of the surgical neck of the bone, and passed downwards and outwards to the lowest point of the attachment of the deltoid. The growth was about the size of an orange; its surface was nodulated and hard; its microscopical structure was that of a spindle-celled sarcoma.

Both these cases were examples of spindle-celled sarcoma, and although the whole of the apparent disease has been removed it is too early to pronounce positively as to future immunity, notwithstanding the complete removal of the bone involved.* In the case of a girl whose thigh I amputated last summer I think we may take a hopeful view of her future, because a section of a tumour showed ossification to be in progress; this I believe to be a favourable circumstance, as I shall show in another remarkable case which was in this hospital some years ago.

CASE 3.—B. B., a girl aged eighteen, was admitted on June 2nd, 1887. She stated that about four years ago she first noticed a painful swelling of the lower part of her right thigh. The swelling steadily increased, and ten months ago the patient came to the hospital, where she was treated as an out-patient with lotions and blisters, but without obtaining any relief. The increase in size still continued, and on admission to the wards the swelling involved the lower third of the shaft of the femur, and ended about two inches and a half above the lower articular surface. The extent of the growth on the anterior surface of the femur was four inches and a half. The circumferential measurement of the thigh opposite the middle of the growth was an inch more than at the correponding part of the other limb, The femoral glands were not enlarged, and no signs of growth in the lungs could be detected. On June 15th an incision was made over the outer surface of the growth on the femur. A portion of the growth was removed by the trephine, and, as it appeared to be sarcomatous, the limb was amputated at the junction of the middle and lower thirds. The vessels were ligatured with catgut, and the flaps united with silver wire sutures, with intermediate ones of catgut. One large tube was placed across between the flaps, and a dressing of iodoform powder and salicylic wool applied. The temperature reached 101° on the second day after the amputation. It then fell to normal, but rose to 100° and 102° on two or three subsequent occasions. The flaps united well. The tube was entirely removed on the tenth day after the amputation, and all the stitches on the nineteenth day after. The patient was discharged on the forty-fourth day.

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* The girl died a few weeks after leaving the hospital, from exhaustion.

The growth almost surrounded four inches and a half of the lower third of the femur, and was periosteal. The base of it was composed of very dense bone, which was continuous with that of the shaft of the bone. The depth of the growth from the surface to the medullary canal was an inch and a half. Its periosteal surface was covered for a depth ranging between a quarter and half an inch with soft glistening tissue, which had a fibro-cartilaginous appearance, and into this rays of dense bone extended irregularly. The microscope showed the growth to be a mixed round and spindle-celled sarcoma, which was undergoing chondrification and ossification.

This patient was seen on October 16th, 1887. She was quite well, and there was no sign of recurrence in the stump.

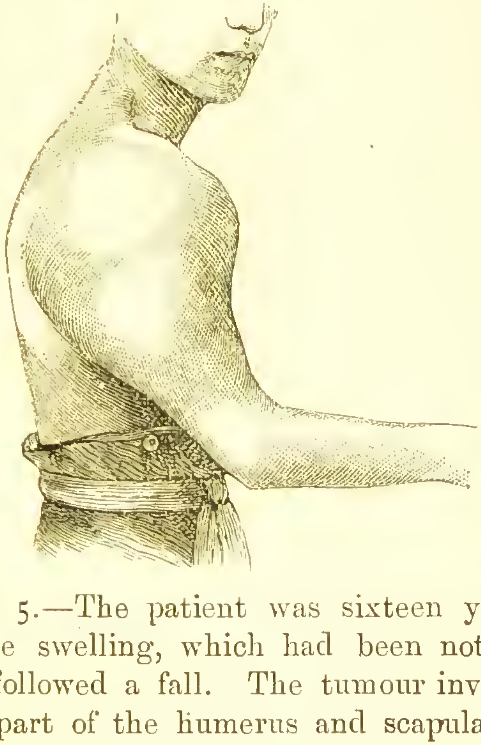
I have taken some trouble to hunt up the patients upon whom I have operated for sarcoma, because I think erroneous deductions as to the fatality of many of these cases have been drawn by those who have collected only a small number of cases. I wish, therefore, to publish all I can, with a view to arriving at clearer opinions founded upon sufficient data. The following is a case in which the patient died two years after the operation, but we could not ascertain whether there was recurrence or not.

Case 4.—R. H., a woman, aged fifty-eight, was admitted on February 5th, 1884. She had noticed a swelling of her right thigh for ten months. On admission the growth occupied the middle two-thirds of the limb; it enveloped the front, the outer, and half of the inner and posterior aspects of the femur. The tumour was firm except in front, where there were two rounded fluctuating projections. The skin was shiny and slightly discoloured, and large veins coursed over the growth. The circumference of the right thigh measured seven inches and a half more than that of the left. The lymphatic glands in the groin were not enlarged. The tumour was aspirated, and two ounces of dark blood-stained fluid were withdrawn. On the day after admission the thigh was amputated just below the trochanters. An anterior flap of skin and fat was cut and raised. The muscles were divided transversely down to the bone, and the latter was sawn. A posterior flap was then cut from within outwards. Shortly before the operation three ounces of brandy and three ounces of water were injected into the colon. The pulse remained good during the operation, but it became very weak afterwards. Frequent small doses of brandy were given during the following six hours. The urine was drawn off every six hours.

The flaps united by first intention, except where the drainage-tube was placed. The tube was removed on the twentieth day after the operation, and the stump appeared to be healed three days later. The temperature kept normal from the ninth day to the twenty-sixth, when it rose to 100°, and four days later to 101°. On the thirty-third day a little discharge escaped from between the flaps, and so a tube was placed at the inner end. The temperature then became normal. The patient was discharged on the thirty-eighth day after the operation. The tumour was growing in the rectus muscle, being limited in front and behind by the fascial sheath of the same. Along the outer surface of the femur, in the upper third of the part removed, the tumour approached very close to the bone, and here the latter was eroded. The growth was a myxo-sarcoma (spindle-celled), and enclosed one large and two small cavities containing mucoid material. This patient died in April 1886.

I now come to a remarkable case of sarcoma of the arm and shoulder, which was under my care in 1883, and is shown in Fig. 3.

Fig. 3.



Case 5.—The patient was sixteen years old, and the swelling, which had been noticed two years, followed a fall. The tumour involved the upper part of the humerus and scapula, and on July 4th, 1883, I removed the whole of the upper limb, with the scapula and outer third of the clavicle. As the case has been already published,* I

need not give all the details, but it will be sufficient to say that the patient made a thoroughly good recovery, and on his discharge presented the appearance shown in Fig. 4. The growth was a spindle-celled sarcoma undergoing ossification, and when I brought the case before one of the Medical Societies, I was very positively assured that the disease would certainly return and kill the patient. Return it did; once seven months and a half after the operation, when I removed a small nodule from the cicatrix; and again two years and a half after the original operation, when I cut a mass out of the pectoral muscle. That is

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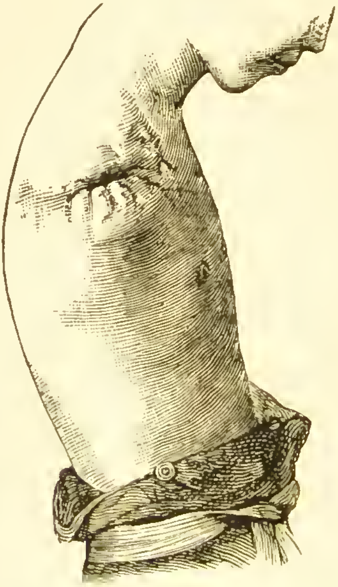
* *British Medical Journal*, July 10th, 1886.

FIG. 4.



now two years ago, and the patient is, as I have recently ascertained, now in good health. This bears out what I said before in connection with Case 3, that cases of ossifying sarcoma are more favourable than others.*

I now show you two living patients whom I have managed to bring here to-day to illustrate two points: the first is the absolute immunity which follows thorough removal of myeloid or giant-celled sarcomata; and the second, the fact that sarcoma of muscle does not always make the rapid and fatal progress which is laid down by some authorities.

CASE 6.—J. S., a woman, aged thirty, was admitted under my care on October 3rd, 1879. She had often suffered from rheumatic swelling of her joints, and about nine months before admission she experienced great pain in her left shoulder, which also became much swollen. The left shoulder was found much enlarged, owing to a tense elastic swelling; the skin over it was hot and a little red. Movements at the joint were restricted and very painful. The patient was discharged a fortnight after admission, and after three months' absence she returned. The swelling was much greater; the skin was red and hot, and the superficial veins were distended; the swelling extended from just above the insertion of the deltoid muscle below to the acromion and coracoid processes above. It appeared to be due to a new growth, and on manipulation, with the aid of an anæsthetic, the humerus was found to be broken at the lower end of the growth.

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* This patient was again operated on in December 1888, and eventually died in January 1891, with further recurrence in the chest-wall.

On January 20th, 1880, the limb was amputated at the shoulder-joint, and a few small pieces of growth, apparently in the subscapularis muscle, were cut away with scissors. The vessels were secured with catgut ligatures, the flaps were united with wire sutures, a long drainage-tube was passed through the wound, and a carbolic gauze dressing was applied. The temperature reached 100.6° on the third day, 101.4° on the sixth, and ranged between the normal and 100.2° until the eighteenth day after the amputation, and then remained normal. The wound healed well, though not completely by first intention. The drainage-tube was removed on the thirteenth day, and the patient was discharged on the thirty-seventh day after the operation.

A tumour about the size of a large orange replaced the head of the humerus, and the shaft of the bone was broken obliquely about an inch below its head. The growth contained a number of blood cysts, and spicules of bone could be felt here and there in its substance. The microscopical examination of the growth proved it to be a myeloid sarcoma. This patient was seen in October 1890.

She was in good health, and there were no signs of recurrence.

The last patient (Case 7) is, as you see, a well-grown girl of eighteen, who has a mass in the adductor muscles of the thigh, but otherwise is in good health, and yet four years ago I abandoned her case as hopeless after two extensive operations. The following are the notes of her case.

Case 7.—A. K., a girl, aged fourteen, was admitted on October 15th, 1883. She had sustained an injury to the inner side of the left thigh eighteen months previously. This was followed by the formation of a hard lump as big as a small apple, which disappeared in about a month after being painted with iodine. Six months ago a tumour appeared in the same situation. It did not attract attention till it reached the size of an orange. Iodine was again applied, and the tumour was thought to diminish in size. On admission there was on the inner side of the left thigh a large pyriform mass as big as a large pear, and reaching to within about an inch and a half from the body of the pubes. The mass was freely movable upon the subjacent structures, and the skin moved freely over it. Neither the femoral nor inguinal glands were enlarged. On the second day after admission the tumour was exposed and incised, and found to be a sarcoma. It extended deeply into the limb, and involved the muscular substance of the adductor longus, gracilis, and pectineus. The growth was removed in three portions, and at one time it appeared that its removal would necessitate amputation at the hip-joint. The patient was very faint, and a hypodermic injection of ether was given; an enema of a pint of water and three ounces of brandy was thrown into the colon. Four vessels were ligatured; a large drainage-tube was inserted in the wound, and the latter closed with sutures. An antiseptic dressing was applied.

The patient was considerably collapsed after the operation, the temperature being 96.2° two hours after. Frequent small doses of brandy were administered. An abundant discharge from the wound necessitated a change of the dressings at the end of three hours. The day after the operation the temperature rose to 102.2° at 7 P.M., and at the same hour on the second day after the operation it was 101.6°. During the next three days it varied between 100° and 101°, and fell to normal by the tenth day, and remained so afterwards. The discharge from the wound was offensive on the fifth day; on the sixth a quantity of offensive pus was found in the wound. The wound was syringed out with carbolic lotion, and the sutures in the centre of the wound were removed. On the ninth day the wound was gaping, and all the sutures had cut out. The wound gradually healed from below, and the patient was discharged on the thirty-second day.

On examination the harder parts of the growth were found to contain bundles of connective tissue and well-defined blood-vessels; other parts were homogeneous and composed of spindle-shaped and oval cells only.

She was readmitted five months later, and stated that the wound had healed about a month after she left the hospital. Three or four weeks after the wound had healed she noticed a lump in the scar. On readmission a firm tumour between the pelvis and the upper part of the cicatrix was discovered to be adherent to the adductor muscles. The growth was exposed by an angular incision, and a triangular flap of skin reflected downwards. The tumour was then peeled off the pubic ramus and dissected from the adductor muscles, but the growth could not be completely removed owing to its proximity to the femoral vessels. The tumour was found to be a fibro-sarcoma, like the original growth. The temperature was 102° the first two days after the operation, and varied between 100° and 101° till the fourteenth day, when it fell to normal. The wound suppurated freely, and finally broke open. It was granulating healthily on the nineteenth day after the operation, when the patient was discharged.

* This patient is alive (May 1891) and in good health.

ON

OVARIOTOMY.

YOU know, gentlemen, that I have lately had here under my care a patient who was sent up to me from the country with the ordinary form of dropsy connected with the ovary—namely, dilatation of the cysts which form the natural structure of the ovary. When she was admitted on February 16th, 1888, she had an enormous abdomen, as you will remember. Although she was only thirty-one years of age, she looked a good deal older. She was somewhat sallow, but otherwise she seemed in very good health. The history of the case was that she had had one child, and her abdomen had been getting steadily larger for nearly a year.

You will remember that we went into the diagnosis at the bedside, and we found that there was very distinct fluctuation over the greater part of the swelling, that there was a dull note over the central portion where the tumour existed, and that there was a clear percussion note on the right side, but one not so clear on the left side.

The left side was somewhat dull, and I ventured to predict that we should find that the tumour had formed a secondary cyst, or more cysts, in the left region, and so it afterwards proved. The fluctuation was so very distinct that it might almost have been thought to be a case of ascites as regards the wave of fluid, but of course the fact that the dulness was in the centre and the clear note at the side negatived that. The only other thing that it might possibly have been was one of those exceedingly thin cysts which we find connected with the broad ligament, which are sometimes no doubt parovarian, and therefore correctly called parovarian cysts, but are better called cysts of the broad ligament; but the size negatived that. You never see a parovarian cyst or a cyst of the broad ligament so large, and I had no doubt that it was a true ovarian cyst.

I got Dr. Williams to see the patient, and as he agreed with me in the diagnosis, the only question, then, was when and where the operation should be done. It so happened that the room in which we are in the habit of doing ovarian operations in this hospital, most of which fall into the hands of Dr. Williams, was occupied at the time, and I did not like to leave the patient unrelieved with such an enormous abdomen, because there is a risk in such cases that some cyst might give way accidentally and a quantity of fluid be poured into the peritoneum, and possibly a fatal result ensue. You will remember that a day or two after her admission, therefore, I tapped her in the median line with a large aspirator-needle—one of the largest size—and drew off slowly thirty-one pints and six ounces of thickish fluid. If you work that out you will see that it is very nearly four gallons, and that will give you an idea of the size of the large cyst which was tapped. It was one cyst only that was tapped; the other cysts, which we found subsequently, of course not being interfered with by the trocar.

One word with regard to the question of tapping. You will find that there are differences of opinion amongst authorities as to the advisability of tapping. No doubt in this case it was advisable, because we wanted to give relief and tide the patient over a few days longer; but if that were not the case, would it be wise or not to tap the patient before operation? I think the general opinion is that it is better not to tap. You will certainly have a little risk in tapping; you might let out a little fluid into the peritoneum, and possibly set up a certain amount of peritonitis, and in that way get adhesions. Again, it is possible that you might disseminate a little portion of some growth out of the cyst itself into the cavity of the peritoneum. Therefore I think you will find that the majority of authorities say that it is better, save in exceptional cases, not to tap. But this was just one of the exceptional cases where it was well to tap, and I do not think it did the patient any harm.

Now to come to the operation of ovariotomy, which I performed upstairs in the isolated room on March 9th. The patient, you will remember, had a perfectly uninterrupted recovery. She was removed downstairs the week afterwards into my wards again, and I then took out all the stitches, and she is now fairly convalescent, and is only waiting for the weather to moderate before going home. Then came the question whether the operation should be done with strict antiseptic precautions or not. It certainly was done with strict antiseptic precautions as regards everything we could do to render our hands, our instruments, and sponges aseptic, but it was not done with a spray. There was a spray ready, and those who were present may remember that I declined to use it, but I allowed it to play away in the room. There can be no objection to that, because it keeps the air of the room sweet, but there is a very great practical objection to having the patient covered with spray at the time of the operation. If you allow the peritoneum to be sprayed upon, undoubtedly you get a certain amount of fluid into it, which is undesirable; and, still more, the spray has a great cooling effect upon the intestines, which is certainly objectionable, for it lowers the patient's vitality, and is apt to produce shock. I think, too, it is better to avoid the possibility of a considerable quantity of carbolic acid being absorbed by the peritoneum. You will remember that the peritoneum absorbs extremely readily, and if you have a spray of 1 in 40 carbolic thrown over it, it is conceivable that you might get more carbolic acid absorbed than is good for the patient.

Let me finish this question of using the spray by quoting a few sentences from a lecture given the other day at the College of Surgeons by Mr. Cheyne, who is a great authority, as you probably know, upon antiseptics. I quite agree with him when he says: "In former times the peritoneal cavity was looked upon as one especially liable to inflame, and it was thought to be one of the triumphs of antiseptic surgery that operations could be performed on the peritoneum without bad result. The experience of a number of surgeons has, however, now shown that it is not absolutely necessary for success in operations on the peritoneum that all bacteria should be excluded from the cavity; in fact, this seems to be much less necessary than where operations are performed on other serous cavities, such as joints, or on the subcutaneous or muscular tissues. The explanation of this surprising result is found in the nature of the lining wall of the cavity, and in the conditions under which pyogenic organisms find themselves there. The peritoneum has marvellous powers of absorbing fluids, and thus effusions into it are very rapidly removed, and in this way micro-organisms are deprived of the necessary nutrient material, while they are also, in all probability, absorbed along with the fluid and destroyed in the blood, or excreted." Then, in another part, he says: "It is evident, therefore, that when authors draw conclusions to the effect that aseptic treatment is unnecessary in surgical practice because they obtain good results in operations on the peritoneum without it, when they take care to introduce as few of these organisms as possible (in many cases probably none at all are allowed to enter), to introduce them in as dilute a state as possible, to remove all the fluid and other materials, such as blood-clot, in which they can grow, and to avoid injury to the peritoneum as far as they can, they make an assumption which is not at all in accord with other clinical and experimental observations. The points which I have mentioned amply explain the results, and bring them into unison with those of experiments and of clinical experience with regard to other tissues of the body." I do not for a moment say that we did the operation without antiseptics: we did use antiseptics, but the point that I want you to understand is that I purposely did not use the spray, and I think the result shows that it was not necessary. There are many surgeons who still use the spray for peritoneal operations, but I think that those cases which you have seen of intra-peritoneal operations where the spray was not used, such as gunshot injuries, and others which Mr. Barker has recently had under his care, most of which have been very successful, show that a spray is not necessary in operations upon the peritoneum, but had perhaps better be avoided.

Then as to the steps of the operation. Those who were present will remember that I simply made an incision three inches long; that I exposed the sac by dividing the peritoneum upon a director, and that the sac immediately came into view—a bluish-looking thin membrane, which is its ordinary condition. There was no peritoneal fluid, and the cyst was at once exposed. I put my hand in and broke down some slight adhesions: and then, as you will remember, I tapped the cyst at once with Wells's trocar. I first tapped the large cyst, that was of course the original cyst which had gradually been refilling, and then I found other secondary cysts, one of which I tapped as well. I also found at the back part, as I expected, some almost solid material, and I thought it better to enlarge my incision slightly, perhaps a couple of inches more, making it five inches altogether. I was then able to deliver the cyst, as we say, to draw it out of the peritoneal cavity, and expose the pedicle. In drawing it out, I found again a few slight adhesions, and you may remember that the omentum was adherent at one or two points. I tore those through and got to the pedicle.

It was, as you see, a wide pedicle, formed, of course, by the broad ligament of the uterus, and you can see here on the cysts the end of the Fallopian tube. I transfixed the broad pedicle with an aneurism needle carrying carbolised silk. I tied it at first in two portions, and then, cutting one ligature short, I used the other to put round the whole of the pedicle, and tied it again in one piece. It is a little important to bear in mind that it is not safe to tie a pedicle simply round as you would an artery. Never mind what the size of a pedicle is, you should transfix it and tie it in two halves by separate ligatures. That may be in a great many cases sufficient, but then it is just possible that there might be a vessel in the middle which might have been transfixed by your needle or torn by the ligatures. It is therefore safer, though it is not essential, to take one of the ligatures and bring it round again behind the point where it has been tied before, so as to enclose the whole of the pedicle again. You will find that some operators lay a good deal of stress on looping one ligature into the other; they say it is not safe to tie the two halves separately, but that you should slip one ligature into the other. I have not done it, and I do not think it necessary, and I have never had trouble in any way afterwards with the ligature. Next I cut the ligature short and divided the pedicle with scissors. Then came the examination of the peritoneum and stopping one or two bleeding points in the omentum, which we tied with fine silk. Then the peritoneum was sponged carefully out and the abdomen closed.

Now, it is in closing the abdomen that Sir Spencer Wells made the great improvement. Previously to his first operations, now thirty years ago, it had always been laid down in surgical works that in any case of wound of the abdomen you should be careful, in putting in stitches, not to include the peritoneum. He found, on a post-mortem examination of one of his early fatal cases of ovariotomy, that, although the upper part of the wound had united, the two peritoneal surfaces were asunder and had not united, and that there was suppuration between them. He made a few experiments on rabbits, the results of which you may see in the College of Surgeons' Museum; and he found that when he carried the needles through the peritoneum and brought the two surfaces together he had at once adhesion of the peritoneum, and the wounds united rapidly and well. Sir Spencer Wells at once published his results, and ever since that the rule of surgery has been reversed, and every one now would put the needles through the peritoneum and bring the two adjacent peritoneal surfaces close together. That is exactly what I did. I used a silk thread with a needle at each end, passed it from within through the abdominal wall, about half an inch from the edge, and put in five sutures, which I tied fairly tightly.

Then I blew a little iodoform on the wound, put a pad of wool over, and plaster over that, to keep the parts quiet.

The patient made a perfectly good recovery. We have here the temperature table. I performed the operation on March 9th, in the morning. The temperature went up a little towards the evening; it was as high as 101° at seven P.M., and it was 102.2° at eleven P.M. The next day it went even a little higher—102.4° and 102.8°; and at three o'clock it was at 103°. Then it gradually came down, and in two days it was at the normal, and there it continued until the patient left the separate ward. With regard to the temperature in these cases, it is said that if you use the spray you get a lower temperature than if you do not use it. I do not believe it for a moment. It is true that I did not use the spray here, and the temperature seems to have gone up a little, but I can show you in some old case-books of mine records of temperature after ovariotomy illustrating this point. Here is the very first—that of a woman on whom I performed ovariotomy in 1870. The temperature is carefully recorded throughout, and it was as nearly a normal temperature as it could be, except on the evening of the operation, and then it was only 100.2°. It never went above 100.6°. And this was before the days of carbolic spray—practically before the use of antiseptics—because we certainly took no precautions in those days to produce aseptic results. We had cleanliness, but nothing more. I only want to illustrate the fact that a spray certainly does not keep down the temperature, and that the rise in temperature in this case was not due in any way to the want of spray. What it was due to I cannot say: it is one of those curious things which happen from time to time. It certainly did not mean any septic poison, because the patient made a perfectly good recovery.

So much for the case; now I want to say a few words with regard to the operation of ovariotomy. And first with regard to the circumstance that I and my surgical colleagues so seldom do it here. It is not our fault, it is our misfortune; we do not get the cases. The fact is that the gynæcological department sees these cases much more frequently than we do, and Dr. John Williams does the operation so well himself that he does not call in his surgical colleagues to assist him. I am not particularly jealous of it; I am quite content that he should have his cases; but at the same time I should be sorry for it to be supposed that ovariotomy is not a strictly surgical proceeding. It is a surgical operation, and it was by surgeons really that ovariotomy was established.

If you go into the history of ovariotomy, you will find it was first done by an American surgeon, Mr. McDowell. I have here a copy of *A Memorial Oration in Honour of Ephraim McDowell, the Father of Ovariotomy*, by Samuel D. Gross, M.D., the leading surgeon in America of his day. In 1879 there was a monument, of which I have here a photograph, set up to McDowell by the Kentucky State Medical Society, and after the manner of Americans they got Dr. Gross—a fine old surgeon—to make an elaborate speech (with which I will not trouble you) dedicating this monument to McDowell. Then, as a reward to Dr. Gross for having made the speech, they did an eccentric thing: they presented him with McDowell's knocker, which used to hang upon his door when in practice; and I have here a short speech made by a poor friend of mine, now dead—Dr. Cowling, of Louisville—in which he speaks in very high-flown terms of the different associations of this knocker with McDowell's practice.

McDowell was, no doubt, the first to perform the operation, and it was occasionally done by various surgeons, the late Professor Lizars, of Edinburgh, being the first who did it in this country. In London it was done by one or two surgeons. Mr. Cæsar Hawkins was, I believe, the first who had a successful case in a London hospital, a great many years ago. The late Mr. Baker Brown, I think, did the most at one time to bring ovariotomy into note, but he had no very great success; and it was not until Sir Spencer Wells took it up in 1858 that the operation may be said to have been established. There were a few cases recorded, but they were mostly fatal, and it was not until about the year 1860 that ovariotomy became a recognised operation.

Now let us see what was the precise operation in the early days. The abdomen was opened, not always in the middle line; I believe in the earlier operations the incision was made on one side. But certainly in the later operations the linea alba was incised, and then the tumour was removed without tapping. That is the great point, that when a large incision was made, sometimes from the ensiform cartilage to the pubes, and the whole mass of the tumour was dragged out, naturally a large quantity of intestines came out with it, and the only wonder is that any of these patients survived. The first great improvement was limiting the incision and tapping the cyst, thus reducing the bulk of the tumour, and so removing one great element of danger.

Then as to the pedicle. The first treatment of the pedicle was exceedingly simple. The surgeon tied it, and he left two long ends of thread hanging out of the lower end of the wound, and after a few days, if the patient lived so long, the ligature came away and was dragged out of the wound with a piece of the pedicle in it. A few cases recovered even by that very rough method. But the first great improvement was the idea that it would be better to bring the pedicle outside, and I think that Mr. Jonathan Hutchinson is entitled to the credit of that. He thought that if the pedicle could be retained outside—I mean the cut end of it—we should avoid a considerable amount of risk of intra-peritoneal suppuration, and he devised what is called a clamp. I really had some difficulty in getting a clamp this morning to show you. The instrument is now so completely gone out of fashion that Mayer and Meltzer could not find a complete one; but here is the end of one, and you can easily imagine that it ought to have two handles. This is the original of Mr. Hutchinson's clamp, which was nothing more than a pair of callipers. The pedicle was put through and squeezed up as tightly as possible, and then the nut was screwed down. The handles were taken off for convenience; the wound was brought together, and the clamp was left outside the abdomen.

There are various modifications of clamps. This was one of Sir Spencer Wells's last, I believe; but I think there is no clamp so good as this simple one, where the edges are parallel to each other and well serrated. Of course, the pedicle being outside the abdomen, any action on the end of the pedicle took place outside the peritoneum, and after a few days the pedicle got loose from the clamp. There was probably no hæmorrhage, the vessels being stopped by the ordinary process. Then the pedicle got retracted a little into the cicatrix, and became adherent there. That was one of the principal objections to the clamp—that the end of the pedicle, being entangled in the cicatrix, was left there, and sometimes remained raw and sore, and occasionally, not unfrequently I believe, it menstruated. The fact is, there was the end of the Fallopian tube included in the pedicle, and when menstruation took place on account of the other ovary you were apt to get a discharge.

Those surgeons who were not satisfied with the clamp tried to divide the pedicle with a cautery. Mr. Baker Brown, I believe, was the first who did it, and he did it rather extensively. I have here two of his cautery clamps. You will at once see how they were used. Supposing this towel to represent the pedicle, it was put through the clamp and the clamp was screwed up tight; in fact, it is very much like a magnified Smith's pile-clamp. These pieces of ivory protected the abdominal wall from injury. Then the cautery was worked against the metal bar, and the pedicle was cut through. Of course, if the cautery acted perfectly, nothing could be better; but practically it was found—I found it myself—that when you came to let go the clamp you were apt to have a little bleeding, one or two vessels spouted, and then you were obliged to apply the cautery again, or to pick up the vessels separately and put ligatures on.

Then we had introduced into this country, by the late Dr. Tyler Smith, the suggestion that perhaps, after all, it would be better to tie the pedicle and cut the ligatures short, leaving the ends in the abdomen, and that is almost the universal practice in the present day. But it is right that it should be understood that it does not at all depend on the introduction of the Listerian method of treatment, because it was done long before that. I remember seeing Dr. Tyler Smith operate in 1868, long before Lister brought out his system, and then the pedicle was "tied and dropped," and the patient did perfectly well. Since the introduction of antiseptics Sir Spencer Wells has abandoned the clamp, and now drops all his pedicles in. In his book on the *Diagnosis and Surgical Treatment of Abdominal Tumours*, I see that he takes more credit than I should for the antiseptic method. Other surgeons also think that his success during the last few years since 1878, when he began to drop the pedicle in and at the same time use antiseptics, has been due rather more to dropping the pedicle into the peritoneum than simply to the use of antiseptics. However that may be, here you have a case before you where the pedicle was dropped, where antiseptics were used but not the spray, and where the patient has done perfectly well.

Though I cannot speak of hundreds or thousands of cases like Sir Spencer Wells, I think it is right that I should give you the result of my own personal experience of the operation. I was attached for a time some years ago to the Hospital for Women at Soho Square. While there I had cases of ovariotomy—not very many. My first case was a very bad one, in 1870. There were two cysts of the broad ligament, and I used the clamp cautery. The patient died. The second case was a woman, aged twenty-eight; there was a multilocular cyst, and I used the clamp. That was the only case in which I have ever used the clamp; and you will easily understand why. I used an ordinary clamp, and thought that everything had done well; but I was sent for some hours afterwards in a great hurry, and I found the patient pale and apparently moribund; the pedicle had slipped out of the clamp and disappeared into the abdomen, and the patient was obviously bleeding to death. I at once opened the incision in the lower part of the abdomen, got hold of the pedicle, and tied it. Unfortunately the patient had lost too much blood, and died some few hours afterwards. In the third case, I tied and dropped the pedicle; but it was a case of cancer, and cases of cancer almost invariably do badly, and the patient died on the thirty-fourth day. One can hardly say that she died from ovariotomy, but she did not eventually recover, and she died really of cancer.

With my fourth case I was more fortunate, the patient making a recovery; but it was a curious recovery, because a singular accident happened during the operation. It was the case of a woman, aged thirty-seven, with very great adhesions, on whom an unsuccessful attempt at ovariotomy had previously been made. The woman came to me, and I thought that, on the whole, I might try and do what I could. I performed ovariotomy, and succeeded eventually in removing a cyst, and she got perfectly well. I will give you short notes of the case from the Clinical Society's records. She was thirty-seven years of age, and the circumstances of the operation were these: "I found it necessary to enlarge the incision upwards, and this I did in the usual manner, raising the abdominal wall with two fingers passed beneath it, and cutting between them with blunt-pointed scissors. On completing the section an unusual appearance at the upper extremity of the incision attracted my attention, and I then found that a piece of small intestine, perfectly empty and adherent to the abdominal wall, had been divided in three-fourths of its circumference. An assistant grasping the ends of the divided bowel, I proceeded to complete the removal of the ovarian tumour, in which I encountered great difficulties owing to the firm adhesions both to the abdominal wall and deeply in the pelvis, and also to the intestines on the right side. The cyst was connected with the right ovary, and there was a broad pedicle, which I tied in two parts with hempen ligatures afterwards cut short, and the same plan was pursued with a long vascular band of adhesions. I now returned to the divided intestine, which I treated on the ordinary principles of surgery by establishing an artificial anus in the following manner." With that I need not trouble you. I made an artificial anus in the wound, and the patient made a complete recovery. I was able to close the artificial anus to a certain extent, but there always was a little fistula left through which a little biliary fluid discharged, otherwise the patient completely recovered.

Now, let me say at once that a wound of the intestine is one of those things that may happen, and I believe has happened, in ovariotomy both before and since my case. I see Sir Spencer Wells mentions having had a case subsequently to this, in which he found that he had torn the intestine; and he had to remove a piece of the bowel, and afterwards united the two portions together by sutures—what we now call the Lembert suture. The patient died, not apparently from anything to do with the accident; for at the necropsy it was found that the intestine was water-tight. In my case I did not venture to do that. I brought the two ends out and made an artificial anus, and I think that, on the whole, it was best for the patient.

To go on with my list, in which there now come some successes. There was a woman, aged thirty-seven, who recovered—that is the case I have just mentioned; and then there was a woman of thirty-six, who recovered; and the next was a woman of thirty with a multilocular cyst simulating uterine tumour—indeed, it was so like a tumour of the uterus that when I opened the abdomen I was in doubt what I should find; I removed the ovarian cyst, and the patient recovered. The next case was a woman of forty-nine, from whom I removed both ovaries with the cautery, and she recovered. The next case was an unfortunate one—a woman of forty, with cancer of both ovaries. I removed one with the cautery and the other with a ligature; but she died. Then I had a young woman of nineteen, with a cyst of the broad ligament. Perhaps I ought to have enucleated that cyst, but it was not done in those days—I am speaking of 1872; I removed the cyst therefore with the ovary, and she got perfectly well. Then in 1873 I had two cases, both of which I treated with cautery, and both recovered. There were eleven cases altogether which I had in the Hospital for Women.

Then, in this hospital, in 1872, I had a multilocular ovarian cyst, which I removed in this theatre, using the spray, and with all the precautions, as far as we could tell, of an antiseptic nature; but the patient died. Then, in 1874, I had here another case, but, fearing to operate in the hospital, I availed myself of Dr. Graily Hewitt's offer to do it in a house he then had in Gower Street, and the patient was carried there. I removed the tumour, and she did perfectly well. Then the last, the fourteenth case, is the one I have done here, and she, I am happy to say, has made a good recovery.

My experience, as I say, is very small compared with that of many other surgeons. The outcome of it is this, that certainly in the present day I think you should use the ligature, tie the pedicle carefully, and drop it in, taking all precautions of an antiseptic nature; but my advice is not to use the spray. It is better that the patient should be isolated, and particularly that she should be operated upon in the room where she is going to remain, so as to avoid all risk of going to and fro.

I think I ought to say that even the best ovariotomists occasionally make mistakes in diagnosis. The late Mr. Baker Brown, who was an extremely able surgeon, used to say that no one could tell what an abdominal tumour was until he had his hand upon it inside the abdomen; and there is a great deal of truth in that. We make elaborate diagnoses sometimes, and we find them quite contradicted when we get inside. I might mention one case which I myself had some years ago, of a patient who had a cyst with very distinct fluctuation, as distinct as in the patient that I have lately operated on. I had another opinion upon her case from a gynæcologist of eminence, who was quite sure that it was an ovarian tumour, and he recommended me to do ovariotomy. I laid the woman's abdomen open, and on manipulation

I found it was not an ovarian tumour at all, but an enormous cyst of the kidney. Then came the question what was to be done. I think, if I had then known as much about kidney surgery as I do now (it was many years ago), I should simply have tapped the patient from behind, closed the abdominal wall, and drained her from behind, much as in the case of the patient lately upstairs in No. 5. But in those days kidney surgery had hardly begun; therefore, recognising that it was a kidney cyst, I proceeded to remove it in the same way that you would do an ordinary nephrectomy from the front. I removed an enormous cyst, and without very much difficulty tied the pedicle, and the patient survived the operation for some days; but she died of a low form of peritonitis, which I believe might have been obviated by drainage. I say so now because we know so much more about these things than we did then. But this is by no means a solitary case. I myself have seen a well-known operator make the same mistake, and find a kidney cyst instead of an ovarian cyst. I have also seen the spleen mistaken for an ovarian tumour, and I suppose it is hardly possible to avoid occasional mistakes in diagnosis. But when you have a case before you, having made a fair diagnosis that it is ovarian, the only way really to settle its nature is to put your hand inside the abdomen, and then deal with it in the way I have described.

ON

TUMOURS OF THE SCROTUM.

I PURPOSE to-day to speak of tumours of the scrotum. It is a very important subject, one which is constantly coming before us here at the hospital, and I need hardly add that tumours of the scrotum pretty frequently come before candidates for examination.

Let me say at once that, having seen a good deal of the examination of tumours of the scrotum by students, it seems to me that a great many of them do not know how to go to work. There is a right and a wrong way of examining a case, and the wrong way is to seize hold of the tumour and put the patient to pain. Naturally, the sufferer does not like it and resents it, and I presume that if he were a private patient he would transfer his attendance elsewhere; he would not put up with it for a moment. The first thing you have to ascertain is whether the tumour is really scrotal, or whether it is abdominal and has come down into the scrotum; of course, that is a matter of vital importance. If it is a scrotal tumour, it is a comparatively simple matter; if it is a tumour which has come down from the abdomen into the scrotum, it may be a very urgent matter, especially if a hernia. This is most conveniently ascertained by sitting down in front of your patient and employing your right hand if the scrotal swelling is on the left side, and *vice versâ.* What you want to do is to feel with the fingers and thumb whether you can make out the spermatic cord above the swelling, or whether there is something obscuring it. If you can feel the spermatic cord, and nothing else with it, it is clear that the tumour must be entirely scrotal, and cannot have come down from above. If, on the other hand, you feel the swelling going right up into the groin, the probability is that it has come down from above. I say the probability, because it is not quite certain even then, for every now and then hydroceles do protrude up from the scrotum, and sometimes even invade the inguinal region; but that is the exception rather than the rule.

These abdomino-scrotal cases may be at once conveniently divided into reducible and irreducible, by laying the patient down and applying gentle pressure. A reducible tumour is, of course, *primâ facie* a *hernia.* When reducible with a little manipulation, it will go up, if it is intestine, with that characteristic "slip and gurgle" which we all recognise as leading symptoms of hernia. But you must bear in mind that you will have the sac left alongside the cord, and that commonly you have the sac considerably thickened in old-standing cases. Again, a hernia may be composed entirely of omentum, in which case it has no impulse on the patient coughing, and the omentum, although it may be pushed back, will go up in a very different way from the slip of the intestine. It requires manipulation really to push it back, and the knotty feel of the omentum is so entirely different from the feel of intestine that you ought, with a little experience, such as you may get in ordinary student life, to be able to distinguish it.

Then, let me say, an omental hernia is very commonly irreducible. It often happens that a piece of omentum has come down and has been so for months or even years, and has got certain attachments to the sac, and the result is that you cannot put it back; it is practically an irreducible hernia. Of course, occasionally intestine is irreducible—not so commonly, I think, in the inguinal as in the femoral region. Still it may be irreducible, and then you would recognise it, of course, not by the slip and gurgle that I have mentioned, but by the general sensation conveyed to your hand, by the feel that it is intestine containing more or less gas, and by the very important symptom that when you percuss over it carefully with your fingers in the ordinary way you elicit a clear note.

Now let us take the other so-called reducible tumours. I should put second *congenital hydrocele* —a form of hydrocele, as you all ought to know, in which the tunica vaginalis still communicates with the abdominal cavity. It is seldom seen except in children, but every now and then occurs later on in life. In a child you get a tumour which you recognise at once as a hydrocele, but it has the peculiarity that, when you lay the child down and hold the scrotum up, the fluid disappears, or, if you have the patient standing, with a little manipulation you can still squeeze the fluid into the abdomen. The way in which that congenital hydrocele returns into the abdomen is quite different from the return of a hernia: there is nothing of the characteristic slip and gurgle, but it is simply a flow of the fluid by gravity into the peritoneal cavity when you hold the scrotum up. It is very important, of course, to recognise this in a child, because, if you make the serious mistake of tapping it without recognising it, it is conceivable that you might be led to inject iodine, and find that you had injected the peritoneal cavity and set up peritonitis.

Then the third form of reducible tumour is *hydrocele of the cord*, which is not strictly reducible, although it appears to be so in some cases. I refer to a little sac shut off from the funicular process of the peritoneum and not absorbed completely, but still containing some fluid. It forms a little tumour, which slips up and down alongside the cord with a little gentle manipulation. If you take hold of the cord and pull it down, you generally pull the tumour down with it, and when you let go perhaps the tumour disappears; still with a little manipulation you can get it down, and then it eludes your finger and slips up again. But there it is always of the same size; it does not vary in the least. The only interest in connection with it is that it is often mistaken for hernia. I have had children brought to me over and over again, even the children of medical men more than once, who were supposed to have hernia. A little care in examination showed that it was not a hernia—that it was simply a tumour containing fluid, and very elastic. You can frequently cure those cases at once by a simple puncture, drawing off a small quantity of straw-coloured albuminous serum.

Lastly, I may mention *varicocele*, since it is reducible to a certain extent, and no doubt is often misunderstood because a large varicocele has a very distinct impulse on coughing. It is a curious thing, but it is only of late years that we have been able to get an impulse on coughing put down and recognised in the books as one of the symptoms of varicocele, although I presume it has existed from time immemorial. Thus, having an impulse on coughing when you make the patient stand up, the varicocele becomes, of course, distended with blood (varicocele, I need hardly say, is a bundle of distended spermatic veins); you can reduce the distension by a little pressure, and in that sense it is reducible; not reducible like hernia, nor exactly like hydrocele of the cord, because you can squeeze the fluid out of the veins, and when you take off the pressure, the fluid comes down again without any straining if the patient is erect. One of the best tests for varicocele is this. You lay the patient down, and squeeze the fluid blood out of the veins; you put the finger firmly on the abdominal ring, and then you let the patient stand up, still keeping the finger on the ring. Of course, if it were anything from above, either a hernia or a hydrocele of the cord, the effect of applying your finger there would be to prevent its filling; but you will find, as the patient stands up, that in two or three minutes the veins fill, of course from below, and there is the varicocele below the finger as full of blood as ever.

We have discussed, then, the four reducible tumours of the scrotum—hernia, congenital hydrocele, hydrocele of the cord, and varicocele; and we now come to the true scrotal tumours. When you have a tumour which is distinctly scrotal, the first thing you have to ascertain is whether it is or is not a *hydrocele*, a hydrocele being, of course, a common affection. We have here a patient who tells me that he was last tapped five months ago, and I see by his paper that he has been tapped five times, and I presume that the sac is now pretty well filled again. It is, as you see, a fairly well-marked ordinary hydrocele. I will proceed to examine it according to my own teaching. First, we have to ascertain whether the cord is quite clear. Here it is perfectly clear, and below it there is a very elastic tumour, large enough in this case to give signs of fluctuation.

Let me warn you, however, against always expecting to get fluctuation in a case of hydrocele. It must be large in the first place, and it must not be too tense in the second, or you will not get fluctuation. But when you have a hydrocele, however small it may be, you will always get the feeling of elasticity which is so characteristic. This is a large hydrocele, and, as you see, I can manipulate it, and almost show you at a distance that there is fluctuation. Then let me call your attention to the fact that it conceals the testicle. You can see nothing of the testicle at all. There is the healthy testicle on the opposite side, but here the testicle is entirely concealed; if you remember the anatomy of the parts, you will know that the testicle is entirely at the back. I have before now heard it said, "It cannot be a hydrocele, because I can't feel the testicle." Now, I should have said that that was the very reason why it was a hydrocele, because the testicle could not be felt. I quite allow that if you know where to look for the testicle, if you feel at the back of the tumour, you can generally get some indication of its presence. I can feel this man's testicle now, and I have no doubt that if I pinched it he would not like it; but if you have a hydrocele a good deal larger than this, the testicle might be so completely overlapped by it that you would not feel it at all.

We have here, then, a very elastic pyriform swelling, which is characteristic of a hydrocele. In order to be sure about it, let me apply the last test—that of translucency. It is here that a great mistake is often made, and I want to show you the proper way of examining a scrotum for translucency. You let the patient stand, and you sit down in front of him. You then have a light held on the side where the hydrocele is. If you put the light on the opposite side, of course there are the other testicle and the penis both in your way; but if you have the light held on this side, you will have it at once close to the hydrocele, and you will have very little difficulty in getting translucency, provided you make the scrotum tense. That is the mistake that is made by a great many persons—the parts are held so unskilfully that the scrotum is not tense, and if you have it loose it stands to reason that you do not get the light transmitted readily through it. If you have any difficulty in the matter, you can easily get over it by using a roll of paper or a stethoscope to look through, and you at once recognise the pink glare caused by the transmitted light.

I will now tap the swelling in order to demonstrate that it is a case of hydrocele, though I have no doubt about it myself. I begin by making it tense; then I plunge in the small trocar and cannula, and at once get the straw-coloured fluid, which I will demonstrate to you contains albumen in large quantity by the addition of a little nitric acid.

Supposing, however, that we had a tumour of the scrotum resembling to all intents and purposes that swelling which I have demonstrated to be a common hydrocele, it might yet not be a common hydrocele after all. If you get an encysted hydrocele growing to a large size, I do not think anybody can be quite sure before tapping whether it is an ordinary hydrocele or not. If it is a *small* encysted hydrocele, you may be quite sure, for you find a small elastic swelling connected with the epididymis, which is the usual position of it, and you tap it with a trocar and cannula and draw off the fluid. I have not the opportunity of showing you a case of that kind, but the fluid is quite different from ordinary hydrocele fluid. It is either perfectly clear water or it is a sort of opalescent fluid, looking very like poor milk-and-water, and it is really the fluid of a cyst developed in connection with the epididymis, and generally containing spermatozoa.

So much for hydrocele. Now for the other tumours of the scrotum. We have in the ward at this moment an old man who has had *hæmatocele* following upon a hydrocele. A hæmatocele is a collection of blood in the tunica vaginalis, which may arise in more than one way. It may be an acute hæmatocele resulting from injury. A man has received some blow, a kick on the scrotum, and almost immediately he finds a swelling. This is partly due, no doubt, to blood effused under the skin, an ordinary bruise, but in many cases you also find a distinct collection of blood in the tunica vaginalis. A recent hæmatocele is much more globular in shape than a hydrocele, as you might expect, for a hydrocele is a very slow affair, and, as it forms, it distends the tunica vaginalis, and gives the characteristic pyriform shape. But if you take an ordinary healthy tunica vaginalis and suddenly distend it, of course it is more or less globular, and so a recent hæmatocele is globular rather than pyriform. In the patient to whom I refer the hæmatocele was distinctly pyriform for this reason: he had a hydrocele first, which had been tapped on more than one occasion, and on the last occasion, for some reason or other, it filled with blood.

Now, people are sometimes uncharitable enough to say that because a hydrocele has been converted into a hæmatocele the surgeon must have made some blunder—perhaps pricked the testicle. I can assure you that it does not at all necessarily follow. I am not responsible for this case, because another surgeon operated; but I have had the same thing occur to myself, and I am sure that I did not prick the testicle. Moreover, I must confess that I have pricked the testicle perhaps on more than one occasion, and to my knowledge hæmatocele has not been produced. The rational explanation of the occurrence of a hæmatocele no doubt is this, that you have got an old patient who has had hydrocele for years, and who has been tapped several times. The veins of the tunica vaginalis and the scrotum are all more or less distended and varicose, and, when you suddenly withdraw a good many ounces of fluid which have been supporting these veins, one of them gives way.

However it may be produced, the symptoms of a hæmatocele are, of course, very characteristic. In the first place, it does not feel so elastic as a hydrocele. It feels more doughy, because there is a large quantity of clot in the sac. Then it feels heavier, because a clot of blood in the sac gives rise to a sense of weight; it is heavier to the surgeon's hand, and the patient says it feels heavier than it did before when it was an ordinary hydrocele. Then, of course, it is not translucent; you cannot make it translucent by any means you can adopt, because no light can find its way through a clot of blood. You may tap a hæmatocele if you like, and draw off the fluid portion of the blood, but, if there is a large clot left behind, the only way is to lay the sac open and turn it out, and let the wound granulate from the bottom.

Presuming that we have no fluid in the scrotal swelling at all, but that it is solid, it must of course be some form of tumour of the testicle. You may, however, have a fluid tumour combined with a solid tumour, and that is what we call *hydro-sarcocele.* The hydrocele must be tapped, and the fluid drawn off before you can thoroughly examine what the sarcocele behind is, and it is well always after you have tapped a hydrocele to examine the testicle and see if it is healthy or not.

Now let us take the solid growths which we have in the scrotum, and first of the acute inflammation of the testicle, which you may meet with either as the result of injury or of disease. A man gets an injury to the testicle, followed, it may be, by acute orchitis; but a much more common event is that he has had gonorrhœa, and that the inflammation spreads along the vas deferens to the testicle, and then he gets acute orchitis. But there is a difference between the two forms. In the case of injury, the body of the testicle is first enlarged and inflamed, and the epididymis is often comparatively healthy. In the case of gonorrhœal disease it is really epididymitis; the inflammation travels first to the epididymis, and when that has become inflamed it spreads more or less to the body of the testicle, and if you see the case early the body of the testicle may perhaps be comparatively healthy. In both cases, let the cause be what it may, you will have much the same symptoms as regards the extreme painfulness of the testis and the redness of the scrotum. The redness of the scrotum is almost pathognomonic of an inflamed testicle, unless the patient has got extravasation of urine, for those are really the two things producing redness of the scrotum. An inflamed testicle is exceedingly tender, and you can almost tell a patient with orchitis, when he walks into your room, by the way in which he straddles his legs and the careful manner in which he sits down on the corner of a chair. In the case of the gonorrhœal testicle, you of course feel the cord, and you find the vas deferens considerably enlarged. But remember that, as a rule, you will not find the gonorrhœal discharge. When the testicle becomes inflamed, the gonorrhœal discharge ceases, for a time at all events; and one of the symptoms of the testicle getting better is that the gonorrhœa is reproduced.

Then, to go on to more chronic forms of orchitis, when you see a testicle with a history of having been enlarged for some months, it is very important to make out what the cause of the disease is. Of course, you may have a syphilitic testicle, or you may have a tubercular testicle, or you may have a testicle which is enlarged simply from irritation in the urethra. It is very important not to make a mistake in diagnosis, and not to jump at once to a conclusion that all cases of enlargement of the epididymis are tubercular. I notice a tendency in the present day to put everything down to tubercle. Well, tubercle has a good deal to be responsible for, no doubt; still it is a mistake to suppose that every enlarged epididymis is due to tubercle, when the patient has perhaps a stricture which will sufficiently account for it.

The first thing is to examine the testicle carefully and see which part is enlarged. If the body of the testicle is healthy and the epididymis alone is enlarged, then it becomes a question whether it is tubercular epididymitis, beginning in the epididymis and spreading from that to the body of the testicle, and also perhaps up the cord; or whether it is a case of epididymitis depending upon irritation of the urethra, in which the inflammation has travelled down the cord the whole length of the enlarged vas deferens. A careful examination of the cord will soon settle that question. You can put a few questions to the patient as to whether he has had gonorrhœa or gleet, whether he makes a good stream of water, and so on. It is not a bad plan, if you have any doubt about it, to examine the urethra, with a conical bougie; and if you find there is a stricture, you had better treat that first, and then hope that the testicle will clear up. The importance of the diagnosis is this, that if it is a tubercular case, mercury will do the patient a great deal of harm; and if, on the other hand, it is a case of chronic inflammation due to irritation, mercury will do a great deal of good. There are cases, I quite allow, where you are obliged sometimes to give mercury tentatively in order to see whether the case will or will not improve under its influence.

On the other hand, supposing we have a testicle enlarged and particularly hard, with very little mischief about the epididymis and none in the cord, and especially if you have a small hydrocele as well, what is the probable diagnosis in that case? In all probability it is syphilitic—late secondary syphilis.

These cases are remarkable for having completely lost that peculiar testicular feeling, as it is called, which is characteristic of the healthy organ. If you take a healthy testicle and squeeze the body of it, the patient will naturally feel an uncomfortable sensation, which we are all familiar with if we happen to give our testicles an accidental squeeze. But the patients with these large testicles have no sensation whatever. You may handle them or do anything you like, and the uniform absence of testicular feeling is very remarkable. On investigation you will always find that some two or three years before the patient has had syphilis. Very likely he was properly treated, but still there is a remnant of it hanging behind; and you must make up your mind that you will not cure that form of testicle without mercury. It is of no use to give iodide of potassium alone; you may combine it with mercury, but I think the better plan is to give the mercury in the form of bichloride. At the same time, you may let the patient rub in some mercury locally, by wearing a scrotal bag, and smearing the scrotum with mercurial ointment or oleate.

Some of these patients, although they have no testicular feeling, have a great deal of tenderness about the testicle, with a sense of weight or dragging, which may be relieved by combining belladonna ointment with the mercurial ointment. There are no cases more satisfactory for treatment than these purely syphilitic testicles. You may be sure that you will cure the patient—that is to say, you will bring the testicle down to about its normal size. But I cannot say that you will be equally sure of restoring the function of the organs, because it may happen that this inflammatory deposit between the tubules may have gone to such an extent that it has become organised into a low form of fibrous tissue; and although you may clear away by means of the mercury all the recent deposit, you may not be able quite to clear up the whole of the fibrous tissue, and that has a tendency to contract and produce atrophy of the testicles afterwards.

You will find in the older books that the authors speak there of fibrous disease of the testicle. But then surgeons did not recognise that this was simply a late stage of syphilis; and there is no doubt that if the deposit has had time to thoroughly organise, the pressure may be sufficient to destroy the tubules, and practically the testicle will be lost. That is the form of syphilis which it is most important to distinguish from new growth; for a youngish man may come before you with a large testicle, and it may be a case of sarcoma, and it is not at all easy always to be sure which disease it is. A good deal will depend upon the history. If there is a distinct history of syphilis, you may, at all events for the time being, give the patient the benefit of the doubt, and hope that it is syphilis, which you may clear up by mercury. If you are in doubt, it is better, unless the tumour is rapidly growing, thereby showing its malignant nature, to give the patient a course of mercury than to remove his testicle unnecessarily. But of course, if you have a rapidly growing tumour of the testicle, the sooner you remove it the better —there is no question about that; whether it is sarcoma or carcinoma, it is better for the patient that it should be taken out.

Then supposing we have a later stage of syphilis. Of course in the later stages we have what is familiarly known as gummatous deposit—that lowly organised form of deposit which we find so commonly in various organs, and particularly those which are vascular. If you think for a moment how vascular the testicle is, you can easily understand how we may have large gummatous deposits in the organ. Here is an example. It is an injected testicle, but the injection has not run very well, and you can see above and below a large mass of yellow tissue which is a syphilitic deposit. Here also is a good example of a large gumma filling up the body of the testicle in two sections, one above the other. This occurs, of course, very much later on in syphilis. You find that the patient has been in good health and getting on perhaps in life, but his health breaks down a little, and he begins to show manifestations of tertiary syphilis in various organs. That is one way in which it occurs, but you may have it quite at the other extreme of life—in children. A syphilitic child will sometimes develop a gumma in the testicle which is apt to be mistaken for malignant disease, because we know that malignant disease also occurs in quite young children, and it is very difficult sometimes to be sure which it is. The tendency of a gumma in the testicle, like a gumma in the tongue or anywhere else, is to break down. You may only see the patient when the gumma has broken down, presenting very much the appearance of an abscess, and involving the skin; or if you see it late enough, the skin may be already open and presenting the characteristic yellow slough which we see so commonly in connection with gummata. We had a good example of it here last summer. I do not know whether you remember it, but I remember the man extremely well. He had double syphilitic disease of the testicle and a sinus and discharge from the gumma in one testicle. The whole thing cleared up under the influence of large doses of iodide of potassium, and he was here the other day perfectly well.

Now, given a case where you have reason to think it is a gummatous deposit, there can be no question as regards treatment. But it is not always easy to be quite sure. I may say at once that I had a patient here some few months ago in a private ward, who had a tumour of the testicle and also a tumour of the arm. He came to my house first, and showed me his testicle, which I thought was syphilitic. I quite made up my mind, and wrote in my notes that it was syphilitic, but just as he was going away he showed me his arm. It looked so like a sarcomatous growth upon his arm that I was shaken in my opinion, and rather came to the conclusion that it was a sarcomatous disease of the testicle, and that he had a secondary sarcoma forming on his arm. He was a very intelligent man, and utterly denied anything like syphilis. He acknowledged that he had had some venereal troubles many years before, but utterly denied that he had ever had anything like syphilis. I took him into a private ward in the hospital, and advised him to have the testicle removed, and there could not have been a more typical gumma of the testicle than there was in that case. It was so good that I took it to the College of Surgeons to be mounted in a bottle, in order that we might have a well-marked undoubted specimen of gumma of the testicle to examine upon. The moment I found the patient had gumma of the testicle, of course it was quite clear what ought to be done for his arm. I put him on full doses of iodide of potassium, and the whole gumma cleared up, and I know that he is now in perfect health. I do not think he was much harmed by losing one testicle. It was a damaged organ, and most likely it would have shrunk up and become merely a nodule ; for this disease of the testicle is apt to lead to extreme atrophy.

Then to go back to the tubercular testicle. Tubercle of the testicle is a disease which appears in the epididymis, and which tends to spread in both directions, upwards along the vas deferens, and forwards to the body of the testicle, and will lead eventually to the disorganisation of the body of the testicle. It is, of course, a deposit of tubercle in a patient who has a tendency to that disease, and it is a disputed point whether the mischief begins in the inter-tubular or in the intra-tubular structures. I myself believe that it is within the tubes that the disease begins, and I think the proof of it is how readily it spreads from the tubes up the vas deferens, and in the opposite direction along the tubuli seminiferi from the globus major to the body of the testicle, and affects all parts of the organ. However, it is a disputed point, and it is not necessary to labour it; but I think that clinical evidence is distinctly in favour of the mischief beginning within the tubes. No one doubts that it begins in the epididymis first, and, as I say, it spreads both ways. In one of the testicles which I removed, and which I show you, we have an example of disease developing in the epididymis and running on to abscesses; but you will see that the body of the testicle is still perfectly healthy, whereas in this other testicle, so far from the body being healthy, the greater part of it is outside the skin altogether, forming what we call a benign fungus or hernia testis.

This disease beginning in a patient with a tendency to tubercle is very often not recognised until there are distinct knots in the epididymis. The patient has no pain, and does not particularly notice it until his attention is accidentally called to it, and then he consults his medical man. In the early stage of the disease, I think there is no doubt that, under very favourable circumstances, absorption may take place; the whole thing may clear up, and the testicle be practically restored to a normal condition. But you require exceedingly healthy surroundings for that. A hospital, I need hardly say, is not the very best place for these cases, and the patients cannot from their pecuniary position take every advantage with regard to their health. But when you have patients of the better class, and you find that a young fellow has one nodule in his testicle, or perhaps two, the proper line of treatment is to send him for a voyage—I mean a voyage to New Zealand or Australia, and in a sailing ship. He should be put under the most favourable circumstances as regards hygienic arrangements—I mean the thing should not be done cheaply, but he should go in a good ship, where he will have a good berth and plenty of good food. You may then find that after he has been away six or eight months he will come back well, the nodule having disappeared. If you get a patient under circumstances which do not admit of such treatment as that, what is the best thing to be done?

It may be said that the best thing would be to remove the testicle. But then you will find that people do not like having their testicles removed, and it is rather a severe operation for a disease which certainly at the moment only affects a very small portion of the organ. I purpose, therefore, in the case of the lad now in Ward 1 to cut down upon the nodule in his epididymis and to scoop it out very much in the same way as you saw m the other day treat the testicles opposite to those before us, in the two patients who are still in the ward. I removed the worst testicle in both cases and scooped out the nodules of tubercle in the better one, and fortunately this youth has only one testicle affected. But then you may say, "Surely the damage done to his epididymis will prevent that testicle being of much service afterwards." Very likely it may; still there is something satisfactory in keeping a portion of the testicle. An individual is not emasculated, although his testicle may not be of very much use to him, if he has a portion of the body of the testicle left. I will not say that he is capable of propagating his species with that testicle, and perhaps it is as well that he should not; still, he has the feelings of a man.

There is another advantage also gained by scooping out the mass in the epididymis: that any secretion taking place in the body of the testicle will find a vent either through the natural tube, or it may be through a small sinus. That is a point of importance, because patients who have nodules in the epididymis blocking it up completely get an uncomfortable feeling of fulness about the testicle, due no doubt to secretion going on in the body of the testicle and not being able to find its way out. The testicle is apt to get enlarged, swollen, and distended with seminal fluid, and unless there is a vent for it the patient suffers a good deal.

Then supposing that we have a case more advanced than that. Let me remind you that the disease spreads up the vas deferens, and that in cases where you have abscesses forming in the epididymis the tendency of the disease is to spread up along the cord towards the urino-genital organs, especially towards the neck of the bladder, to form one or two nodules in the prostate, and to invade the vesiculæ seminales, leading eventually, in the later stages, to most painful ulceration about the neck of the bladder. Let me remind you of the case of a man we had here from South Wales at the beginning of the session. He had both testicles even more advanced in disease than these are. On examining the rectum with the finger, I found he had most extensive disease about the neck of the bladder. The prostate was diseased and the vesiculæ seminales, and altogether the man was in such a low condition that I refused to operate on him, and sent him home again. I am quite sure I did right. If I had done anything for that man probably he would have died, but now he will go home and die among his friends, instead of dying here as the direct result of an operation.

I show you the testicle of the man in whom I did interfere, and which was practically disorganised. Let me remind you that the opposite testicle had a large mass in the epididymis, and I ventured to treat that by cutting down upon the epididymis through the opening made for castration and scooping out all the disease I could find. He has done very well, except that he had an abscess in his inguinal canal, I have no doubt from the height at which I tied the cord. That I opened last week, and the patient is now doing exceedingly well, and seems likely to make a good recovery. This is a good example of the disease creeping up the cord without affecting the body of the testicle at all, for I may point out to you again that the body of the testicle is perfectly healthy.

The specimen from the last case, which I operated upon last Wednesday, shows quite another condition of things. This man had disease spreading up the cord, for the vas deferens is enormously thickened; but unfortunately the disease had also crept forward in the body of the testicle, setting up suppuration and leading to the destruction of the tunics, and eventually to protrusion of this mass, which is made up of the seminal tubes, together with some granulation tissue. Let me point out how completely the whole body of the testicle is outside the skin, forming this fungus, whilst behind we have really nothing more than the epididymis. Now, under those circumstances, there could be no question about removing it. I removed this testicle, and I dealt with the opposite testicle in exactly the same way that I dealt with the former one—namely, I cut down upon the epididymis and scooped out all the disease I could find there.

That patient also is doing well, but unfortunately he has some enlargement about his prostate and vesiculæ, though not so advanced but that I thought I should be justified in doing the best I could. I am afraid I have not cured him, though I am sure I have given him relief, and he may tide on for some considerable time.

Now let us follow in imagination what will be the outcome of these cases. Very probably they will each have a fistula left in the scrotum. That is a very small matter, and can be treated by injections if you like, but I do not know that there is any very great object in troubling about it. But I am afraid there is in one of these cases already disease about the prostate and vesiculæ which will steadily make progress, and probably before many months are over the patient will complain of a good deal of pain when he micturates, and he may have great frequency of micturition, and the two things together will wear the patient out. It is one of the most trying conditions a patient can be in to have tubercular disease about the neck of the bladder, which leads eventually to ulceration of the mucous membrane of the bladder. In fact, the two diseases go on together; and sometimes perforation takes place, and then we have in addition the annoyance of a urinary fistula. At other times that does not take place, and the patient goes quietly on until he is worn out by the constant suffering.

We do not happen to have had a case very lately of sarcoma of the testicle. Until of late years sarcoma was not differentiated from carcinoma, and therefore all the older cases of sarcoma are put down as cases of carcinoma. Sarcoma of the testicle exists in the form of round-celled or spindle-celled, but is more generally, I think, of a mixed variety, and you will find that it is a rapidly growing tumour which has a tendency to form cysts, and that is important, because these cysts in the testicle in former days used to be called 'cystic disease of the testicle.' There is no doubt that most cases of so-called cystic testicle are really cases of sarcoma with cysts developed; and the cysts seem to be developed very much as renal cysts are from obstruction of the tubules. The tubules cannot empty themselves because of the sarcomatous growth pressing upon them, and thus they become distended with their secretion, and form a series of cysts. Now a rapidly growing tumour of the testicle of that kind cannot be removed too soon; the thing should be done without any hesitation at all, and yet you never can guarantee that the patient shall not have further trouble. I remember an officer from India, a few years ago, from whom I removed a testicle which was distinctly cystic, but which was also sarcomatous. He got perfectly well and went back to India, but died within two years with growths in the neck and chest, and with just the ordinary history of sarcoma developing in other parts of the body.

The form of cancer that you meet with is generally encephaloid. Scirrhus is extremely rare, but encephaloid cancer is not very uncommon. In these cases of encephaloid cancer we have very rapid growth; there is also a tendency to fungate, and the fungus is the true fungus hæmatodes of the older surgeons, by which I mean a large bleeding mass protruding from the scrotum, very different from the benign fungus, which is perfectly callous, and does not bleed at all. It is a great misery to the patient, and tends of course to shorten his life by the irritation which it sets up. In these cases of malignant disease the great tendency is to deposit in the glands higher up. You find the whole cord thickened and the glands in the lumbar region involved. You ought to remove these testicles in all cases as soon as you see them. Still, you may be pretty sure that the patient will die some months afterwards from deposit in the internal organs.

ON

FRACTURES OF THE UPPER EXTREMITY.

I AM going to speak to-day about fractures of the upper extremity, for this reason. Fractures of the lower extremity are constantly in the wards, and therefore I have the opportunity of teaching practically upon them ; but fractures of the upper extremity, as a rule, are not admitted into the wards, and it may happen that you do not see them ; they are necessarily treated in the surgery at the time of the accident by the house surgeon, and the surgeons of the hospital do not see the cases at all. I am desirous, therefore, that you should hear my views upon the treatment of this particular class of cases, and I am the more anxious because you will remember that the other day we had here from another hospital a boy who had a badly united forearm. I brought him into the theatre on a Wednesday, put the bones as straight as I could, and in your presence applied the necessary splints and bandages; and I want to give you a few hints to-day as to the treatment of fractures of the upper extremity, in order that you may avoid similar errors.

In the diagnosis of fractures generally, I would put the order of symptoms thus:—First, *deformity*, for in the great majority of fractures of the upper extremity there is a deformity which is tolerably obvious. Next I would put *loss of power*, for the patient has lost the use of his limb, and you can see that he comes to you carrying his broken limb in the easiest position, and complaining if it is in the least jarred, or shaken, or touched. Next therefore, to the deformity and loss of power I would put *pain*. When you have these three symptoms, you may be tolerably sure that you have a fracture to deal with. Still, you cannot be quite sure till you come to manipulate slightly, and as soon as you do this, in the great majority of cases you will find at once the well-marked symptom of *crepitus;* but put crepitus last, if you please, not first, because it is only to be ascertained by the surgeon when he comes to manipulate the part, and he ought to have made up his mind pretty well as regards the nature of the case without touching it at all. Where a fracture is obvious, it is absurd to be pulling it about with the view of confirming a diagnosis which you have at once made for you. Why do you want to pull about the patient and put him to pain till you are prepared to set the limb and put it up? Of course you may put it into shape then and there, but you have no splints ready, and you must let go, and it immediately goes out of shape, and you put yourself to the trouble and the patient to the pain of a second reduction. You should therefore wait till the splints are ready, and then at once reduce and put up your fracture. With regard to that, I am sorry to say that I am quite sure, from the badly set fractures one sees from time to time, that surgeons do not always recognise the necessity of *setting* fractures. They are too apt to clap the splints on anyhow, and to support the limb in the position in which it is. The position may be a right one or it may be a wrong one, and if the parts are deformed it is your business to reduce the deformity before you put on the splints, and then to put the limb up in the best possible position.

After these few preliminary remarks, let me say with regard to the treatment of fractures generally, that if there is one thing more important than another, it is that the limb shall not be unduly constricted. Therefore you will find that all modern books lay down the rule that no bandage should be put next the limb. I am the more particular in saying this because in books of comparatively recent date there is still the ridiculous advice to put a bandage round the limb under the splints. Now if you do that, if you bandage the limb beneath the splints, what must happen? Why, the limb is likely to swell; you have got it constricted by the bandage, and the result is that you sometimes unfortunately get gangrene of the limb produced. Such an accident as that is most disastrous to all parties—in the first place to the patient's limb, and in the second place to the surgeon's reputation, because naturally he will be blamed for such an untoward result.

The case is very different with regard to supporting a bruised limb properly by applying a certain amount of elastic pressure. The late Mr. Gamgee, of Birmingham, the inventor of the extremely useful elastic tissue which I strongly recommend to your notice—the Gamgee tissue—was very strong indeed about the necessity of applying judicious pressure to bruised parts, and he was perfectly right. If you have a bruised limb, and you raise it vertically, the blood flows out of it, and you can maintain that condition by the application of judicious elastic support; the result is that you very much diminish congestion, and, of course, congestion is the first stage of inflammation. How are you to apply this judicious support and pressure in that way without undue constriction? It is to be done by padding your splint thoroughly, and then by applying your bandage over the splint. If you do that with an average amount of judicious care, the result can only be satisfactory.

Before going further, let me say a word with regard to splints. We appear in this hospital to be in the habit, for economy's sake, of cutting splints as they are wanted, from a quarter-inch deal board. Now that is very objectionable, and I hope we shall stop the practice ; because, if you have a perfectly flat board, however carefully you may pad it, when you come to apply it to a convex surface—and you will remember that a limb is always more or less convex—it stands to reason that there must be a tendency to produce undue pressure upon the limb. And there is no necessity to adopt this plan, because a splint almost as cheap, and certainly a great deal better, is the split boarding known as Gooch's splint, like that which I hold in my hand. Or, still better, you can have it cut up ready for you in a series of splints, called by the instrument makers "lined splints." The great advantage in them is that you have a certain amount of giving of the splint. The wood being divided and bending upon the lining of leather, the result is that it can be applied readily to the convexity of the limb, instead of being a flat board which presses unduly upon the part.

You may have all sorts and varieties of splints. Here is one which I use a great deal in private. It is called the Crimean splint, and it is very convenient because it is already padded. As you see, there are three pieces of wood sewn upon the pad ; or, if you prefer it, you may have three pieces of metal. These are reasonable and comfortable splints because they yield to a certain extent in the transverse direction, and at the same time they are rigid in the length of the splint. Then there are splints which are moulded with certain objects. There are, for instance, special splints known as Carr's splints, to which I will afterwards allude more particularly. There is also a useful kind of splint for certain cases—for example, splints which are moulded to fit the hand. Bear in mind, however, that every splint must be padded. It has happened to me before now, I am sorry to say, to take from an arm a splint with no pad whatever. It is in padding splints that the Gamgee tissue comes in so usefully; you can take a piece of the tissue and cut it to fit any splint you like. If one layer is not sufficient, you can make it double or treble at certain points where you want to have special protection to the bone.

Now let me run through the varieties of fractures and point out to you the methods I recommend. I can only give you my own individual views, and I am aware that they are not quite the same as those of some other surgeons, but you will take them for what they are worth.

Take the case of a man who has a crushed finger. The accident is sufficiently obvious. The great drawback is that, unfortunately, in most cases of crushed finger the injury is compound.

Still, supposing you have a simple fracture of a phalanx, what can be simpler than to put a narrow piece of wood of the kind I have mentioned wrapped in cotton-wool or some Gamgee tissue and carry it into the palm of the hand, and then with a strip of plaster bind it round, keeping the finger straight? But, supposing you have a number of fingers fractured, it is not quite so easy. Then you will find it advantageous to have a splint, which you can make for yourself if necessary, cut with divisions in it for the five fingers. Unless you have each finger separately bandaged in this way, they are apt to be jammed together, and you cannot be sure that you will maintain each finger straight.

Taking next the metacarpal bones, I know no splint so convenient as the moulded hand-splint reaching up to the first phalanges. Here you have all the metacarpal bones well supported, and there is no difficulty whatever in the treatment. There is this great advantage, too, in the splint, that the fingers are able to be left out; therefore they do not get stiff, which is a very important thing.

Before leaving the hand, let me remind you that the metacarpal bone of the thumb is not the same as the metacarpal bone of the fingers. The metacarpal bones of the fingers, you will remember, have their epiphyses at the distal ends; the metacarpal bone of the thumb, as regards development, is a phalanx, and therefore the epiphysis is at the base. It happens in the case of children every now and then that there is a separation of the epiphysis of a metacarpal bone; and in the thumb it will be at the base, in the fingers it will be at the head.

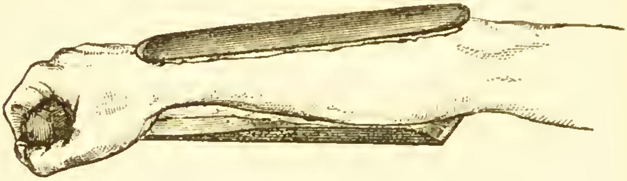
Now to come to the forearm; and I may as well point out what the development of the forearm is, as we have a specimen here. Let me remind you that we have an epiphysis at the lower end of the radius, which includes the whole of the articular end. Then we have a very much smaller epiphysis at the upper end or head of the radius, which includes only just the articular surface. The large epiphysis does not thoroughly unite with the shaft of the radius until the age of eighteen or twenty, so that up to that age you may readily get a separation of the lower epiphysis of the radius, and that no doubt is a form of accident which is much more common in young people than is generally supposed. When you hear of what is termed Colles' fracture occurring in a young person, you may be sure that it is not Colles' fracture at all, but a separation or fracture of the lower epiphysis. The same thing applies to the so-called dislocation of the hand at the wrist. It is an uncommon accident, but when it is supposed to occur in a young person it is generally a separation of the lower epiphysis of the radius. With regard to the ulna, the development is not so important surgically. Here we have an epiphysis at the upper end, and sometimes you find that the olecranon is still separate. There is also a lower epiphysis, exceedingly small, including the styloid process.

Coming to common fractures of the forearm, I suppose one of the commonest is Colles' fracture. I hold in my hand a cast which shows it very well. There is the abduction of the hand and the prominence of the lower end of the ulna, but sometimes there is a much more considerable deformity. You see there is here no displacement of the lower fragment on the back; it is more of a curve, and that is often the case. Here is a capital dried specimen in which you have the lower fragment of the radius carried backwards quite upon the upper surface of the shaft of the radius, and you see it carrying with it the hand. The fragment is carried so completely away from the shaft of the bone that we have two projections, the lower one which is due to the fragment, and the upper which is due to the shaft of the radius. We have also some abduction, but not so much as we often get; consequently you see here the styloid process only a little more prominent than it should be.

Colles' fractures vary considerably; they are very commonly comminuted, and occasionally they are impacted—I should say more often than people think. When there is very considerable deformity, but you find no crepitus or very obscure crepitus, you may make up your mind that it is an impacted fracture. As a rule impacted fractures are best let alone, and anybody, for instance, who breaks up an impacted fracture of the neck of the femur is a bad surgeon. But Colles' fracture of the radius is an exception to the rule; it is the place in which one is justified in breaking up the impacted fracture and restoring the contour of the limb. The way to do it is this: you manipulate the wrist rather forcibly (sometimes it is wise to put the patient under chloroform first), and you over-extend the hand so as to unlock the fragments, which should be done with care. Having unlocked the fragments, you are then able to draw the hand down and bring the bone into a very fair position. I quite allow, however, that there are impacted fractures of the radius which you cannot unlock, and therefore you may be obliged to let the patient go with the impaction and the consequent deformity left as they were.

Supposing you can unlock the fragments and get the bones into a tolerably good position, what is the best way of treating them? In many hospitals Colles' fracture is treated simply by two straight splints, and the hand is put up a little adducted. It is not quite a satisfactory method, and a much better plan is to use the old-fashioned pistol splint, the hand being first adducted and fixed in position, and then the splint brought up to be attached to the forearm. But what I strongly recommend you to use, if you can get them—and they are not at all expensive—are these Carr's splints (Fig. 5).* Carr's splint, which is an American invention, has the great advantage that it has, in the first place, a thickened portion against which the radius rests, and then that it has an oblique bar which the fingers grasp, and on which they can be moved to a certain extent. The bandage is put on so as to retain the fingers for the first twenty-four hours and prevent their movement; but at the end of that time you can take off a piece of the bandage as far as it binds down the fingers, trusting to the patient holding on sufficiently to the bar and moving the fingers from time to time, so as to prevent the gluing together of the tendons, which is so apt to take place. If you see Colles' fracture treated in the ordinary way, patients are perhaps three months or longer before they really get the use of the hand; but if you put on a Carr's splint, at the end of a month they can move their fingers quite

FIG. 5.



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* The illustrations are taken from the author's *Minor Surgery*, by permission of Messrs. Churchill.

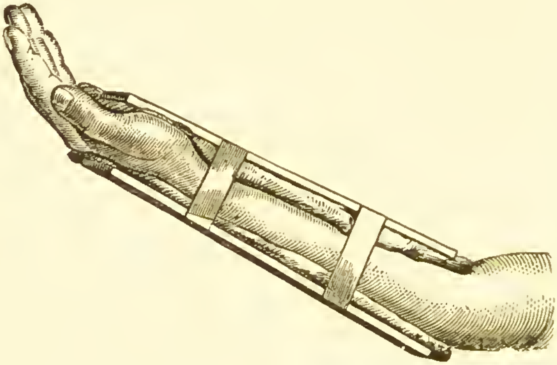
comfortably. The Carr's splint has a straight splint which goes on the back of the forearm; but it is not essential, and I seldom use it.

Now let us come to the shafts of the two bones, and consider what is the best position in which the limb should be put up. When we have a forearm in supination we have the two bones parallel to one another. I do not say anything about the interosseous space, because that is a matter of indifference, but let us consider how we may have the bones parallel when you are setting a fracture of the forearm, and then how it will be most convenient for the patient afterwards to carry his arm.

I will take a couple of Crimean splints, which are very convenient, and apply them to the patient's arm. But how long should these splints be? There we come at once to an important question. I am sorry every now and then to see patients walking about, who have evidently been treated at some hospital, with splints coming down to the tips of their fingers. I know it is unnecessary, because I am sure it is a case of simple fracture, and I know what will happen: the patients will have very stiff fingers for many weeks. What you want is to keep the joint next beyond the fracture stiff and quiet; as long as you do that you may let the other joints alone with advantage. Therefore, when you select your splints for a fracture of the forearm, you ought to make them come down sufficiently far to keep the wrist-joint quiet; but the less you interfere with the metacarpals and the phalanges the better for the patient.

We have here a patient who is supposed to have a fracture of both bones of the forearm. I now place his forearm in supination upon the longer of the two splints, manipulate the radius and ulna, and endeavour as far as I can to get the bones parallel to one another. I then apply the shorter splint to the front of the forearm. It must not be too long, for you want it long enough to come well up the forearm, but at the same time not to prevent the patient bending the elbow. Here, then, we have two bones parallel to one another. I grip the two splints firmly together and turn the forearm midway between pronation and supination, since that is the way you want eventually to get it; but I am sure you do it much better in the

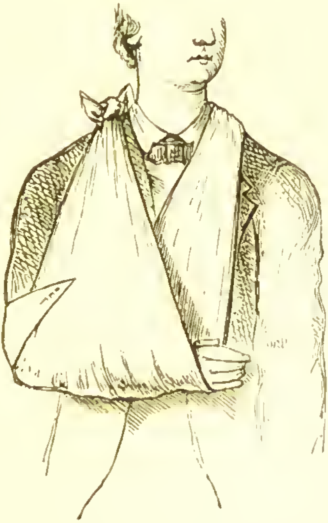
FIG. 6.



splints than out of them. Having got the hand midway between pronation and supination—*i.e.*, with the thumb upwards—I advise that there should be a strip of plaster applied above and below; lastly, the forearm is bandaged, and the patient is ready to go away with it in a sling. (Fig. 6.)

I find on examination nights that gentlemen are sometimes not at all clear as to the different kinds of sling. There is the triangular sling, which is the best for the forearm; and there is the cravat

FIG. 7.



sling, which is the best for the hand. If you have a man with his forearm in splints, what you want is a triangular sling, and you can fold an ordinary silk handkerchief in the form of a triangle for the purpose. The great secret to remember is that the straight edge or base of the triangle goes in front. (Fig. 7.) If you fix that in your memory you will never be in any doubt as to how to put a sling on; and in these days of ambulance lectures some of you gentlemen may get your eyes wiped severely by some young lady, who will know how to put a sling on, and show you if you do not. If you have a better class of patients to deal with, who can afford to have a proper leather sling, you can get one easily enough at the instrument makers. The curious thing is that they do not always know how to put it on, but simply hang it round the patient's neck. The proper way is this: Instead of hooking the sling round the neck, you undo the strap, and, having laid the arm in the sling, you carry the padded strap over the opposite shoulder and across the back, bringing it through the axilla of the affected side. It makes all the difference in the comfort of the patient, for you can easily conceive what it is to have a sling constantly over the nape of the neck for twelve or thirteen hours at a time.

We now come to the upper end of the ulna, and the fracture which is known as fracture of the olecranon. There is a man here who has a fracture of the olecranon, and you will notice that his elbow has been kept at right angles by my direction. Supposing you have a fracture right through the olecranon into the joint, which is the common form, you have then, of course, fibrous union afterwards, and the theory is that if you put the arm out straight you will get a shorter fibrous union and a better result. I do not believe it. You will find that if you let the patient bend his elbow there will be a little separation, no doubt, but not much; and the comfort of having the arm bent far outweighs the other matter altogether. I have seen the most disastrous results from keeping the limb straight. Some two years ago a handsome, well-built young officer came into my room, and asked me to look at his arm. It was perfectly straight and withered. He had no sensation whatever in his fingers, and his elbow and wrist were perfectly rigid. I said, "What has happened?" He replied, "I had an accident from polo in India; it was a simple fracture, and it was treated by the doctor there." It had been put up straight, and it was so badly put up that inflammation ensued, sloughing took place of the whole front of the arm, and destruction of the median and ulnar nerves. This young fellow, twenty-five or twenty-six years of age, was completely disabled for life by this unfortunate treatment of his arm. There was not much to be said; but it was quite clear that nothing could be done except amputation, which I advised, but he declined.

An elbow-joint which has a fractured olecranon is certainly liable to inflammation; there is always effusion in it. (You will remember that in the man's elbow which I showed you, although the fracture was ten days old, there was still considerable effusion.) I am sure that to put an arm in such a position that it may become useless to the patient is quite unjustifiable. What you ought to do is to put it at right angles—in fact, to put the arm in a sling. It is said that you may have a fractured coronoid process. No doubt such a thing might happen, but it is very rare except in cases of smash of the elbow. It does happen occasionally with dislocation of the forearm backwards, but under these circumstances you may disregard it. A fractured coronoid process never unites, so far as we know, with the ulna, therefore there is not much object in treating it; you would only treat the general swelling, and very likely would not know that the coronoid was broken off until afterwards, when the swelling had gone down.

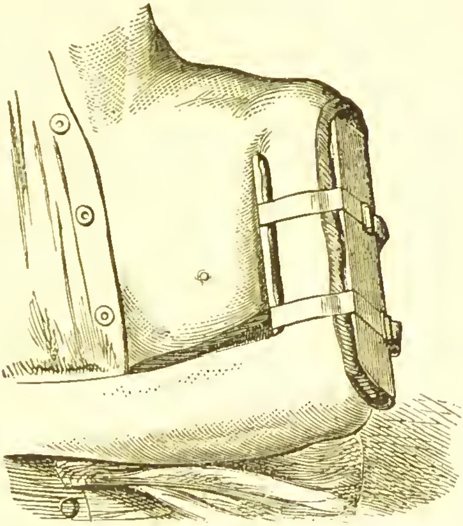
Now we come to the humerus, in which we have fractures of the upper extremity, of the shaft, and of the lower extremity. Fractures of the shaft are comparatively simple matters, but fractures of the upper and lower extremities are usually very complicated accidents, and I suppose there is no class of case which gives more anxiety to the surgeon than a complicated fracture of the elbow-joint, or about the shoulder-joint. We have, as you know, in the hospital a very fine young fellow whose elbow I excised the other day. That is a good example of a complicated accident at the lower end of the humerus. A year ago he received an injury in a football match. It was not clear what the accident was at that time, but I have no doubt, from what we saw afterwards, that it must have been dislocation backwards of the ulna, with fracture of the lower extremity of the humerus. You will remember that he came here soon after Christmas with a very stiff arm, which I tried to get into use for him by forcible bending. He came again in the summer, when he was not at all better, and I then advised him to have the elbow excised.

The great point in a fracture low down no doubt is, in the first place, if practicable, to make an accurate diagnosis (which is not always possible), and at all events to treat the case in such a way that no harm may result. I say that particularly, because I am sure that sometimes harm does result from treatment. I believe that in all cases of fracture of the lower end of the humerus the elbow should be well bent. I include, of course, cases of separation of the lower epiphysis, and I may remind you that the lower epiphysis of the humerus includes all the articular surface, and does not unite with the shaft till about eighteen years of age. I like to bring the forearm thoroughly up so as to put the hand to the opposite shoulder, and then I feel confident that the fragments will fall into the best position. If there should be any stiffness, as there is likely to be, still I believe on the whole it is the best position.

Children will occasionally fall down and hurt their elbows, and at first you may be not at all clear what the nature of the case is. You ought first to make out whether there is a dislocation, because let me remind you that dislocation of the elbow is common in children. The elbow is such a shallow joint in a child that the ulna easily slips backwards off the humerus. But supposing that it is not dislocation, and that it is fracture, then, without making too minute a diagnosis, I would treat all such cases with the arm well bent in the way I have shown you. The great advantage is that if the parts unite a little wrongly you can pull the arm down and restore movement; but it is not so easy to bend the arm up; you are liable to break the bones. Supposing you have a separation of the epiphysis, the treatment will still be the same; but I think it right to warn you that the callus thrown out will be excessive, and will very much limit the range of motion. I mention this because you should always hold out to the patient's friends that things will improve in time. The callus, which is superfluous at first, becomes absorbed, and then there is a fair amount of movement in the joint. But, if you are wise, in all cases of injury above the elbow-joint you should not give a too hopeful prognosis at first.

The shaft of the humerus of course you can easily enough treat by straight splints—two or three straight splints down the sides; but I do not advise you to trust to straight splints alone, because we know that the humerus is the bone of all others which is liable to non-union or false joint. What is the reason of this? It is because

Fig. 8.



of the difficulty of fixing either the elbow or the shoulder. I believe it is impossible to fix the shoulder-joint. You may try any apparatus you like, still the patient will wriggle the shoulder about. It is, however, clearly possible to fix the elbow, and therefore you ought in all cases of fracture beyond the middle of the shaft of the humerus to combine treatment by a rectangular splint with one or more straight splints (Fig. 8).

You bind the three together, and the great advantage is that the elbow is absolutely fixed. If you do that, I believe you will never have to regret imperfect union.

Then coming to the upper end of the humerus, let me remind you of the fine man we had here two or three weeks ago. He came here to show me his arm, and to know if anything could be done for it, simply because it was shorter than the other. He had evidently had an accident which had been treated—I do not say badly treated—and the result was that the humerus was about three inches shorter than on the other side, which of course somewhat disabled him. The explanation of the deformity in fracture of the upper end of the shaft is that there is a tilting of the upper fragment by the supra-spinatus, and a drawing up of the lower fragment into the axilla, partly by the pectoral and deltoid, and partly by the triceps. If you leave the fragments in that position they can hardly unite, or if they do you will have a considerable shortening unless you bring the fragments down into their places. It was because that difficulty had not been overcome in the patient I refer to that he had such a shortened arm. I recommend you to use a splint with a crutch, which you can get made or manufacture for yourselves, if you take a piece of padding and fold it over the top of the splint, and get the arm fixed upon it. You can also put a shield, made of gutta-percha or leather, over the side of the shoulder and bring it down the arm. That is no doubt an extremely good plan, but I can tell you a better, which is to use the ribs and the side of the chest as a splint, and not to have any other splint inside the arm at all. You can put the folded pad up into the armpit, and then put a good padding inside the arm and bring it to the side. I believe that, after all, is the best plan, but you can still, if you like, put a shield or cover to the shoulder. I like to have it open, so that I can see the condition of the parts, but I do not object to the cap.

With regard to the shoulder, let me remind you that we have occasionally a non-union of the acromial epiphysis, of which I have here a specimen in a bottle. That is a little point to bear in mind, because no doubt many of the cases of so-called fractured acromion are loose epiphyses which have never united, and never will. They are treated as fractures, which does no harm, but it is as well to know that the epiphysis may be loose, and that there may be no fracture at all. In the case of a fall upon the shoulder, the most likely thing is to have dislocation at the outer end of the clavicle, and you had better make up your mind to leave that unreduced. There is no instrument or contrivance which will keep the outer end of the clavicle and acromion in proper relation when once dislocated, and you had better say so to the patient. It is a very little deformity, it does not interfere with the use of the arm, and you will only be putting your patient and yourself to a great deal of useless trouble if you try to keep it in place.

Then we come to the well-known *crux*, the treatment of a fractured clavicle. I suppose there has been more misspent ingenuity with regard to the treatment of fractured clavicle than there has been with regard to any other bone in the body. I think that, on the whole, the method we adopt in this hospital—viz., Sayre's—is the best; but it is not the only way. Here is a plan which answers extremely well—I mean by Ellis's apparatus. It is rather cumbersome and a little expensive, and we therefore cannot use it in the hospital. It consists essentially of a good thick pad, which keeps the arm away from the side at the upper part, and throws outwards the shoulder to a certain extent. We have then the upper arm kept to the side below the pad by a broad strap, which encircles the body, and the forearm can be put into a common sling. It is not a bad method; but I think you can get the same results in the great majority of cases simply by padding the axilla and binding the arm firmly to the side.

Sayre's is perhaps the best method; but it is a curious fact that few people know how to put Sayre's bandage on. You want a strip of good plaster, which you loop round the arm and secure with a pin; it goes round the upper part of the humerus, and then passes across the back and is fixed round the body (Fig. 9), being again pinned so that it is held firmly. Now what have we done? We have drawn the upper part of the humerus backwards, and that is the point that Sayre so much insists upon; we bring it back in this way, and make it the fulcrum. For, if we now bring the elbow a little forward, you will see that the humerus pushes the shoulder—that is, the outer end of the clavicle—backwards and outwards, which is exactly what is wanted. Having thus fixed the arm, we hold it by another strip of plaster, which has a hole for the elbow cut in it (Fig. 10). Then it is wise to put a third

FIG. 9.

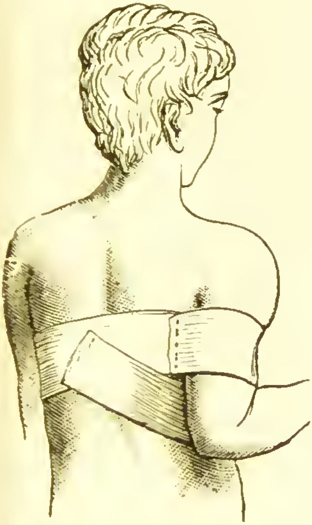
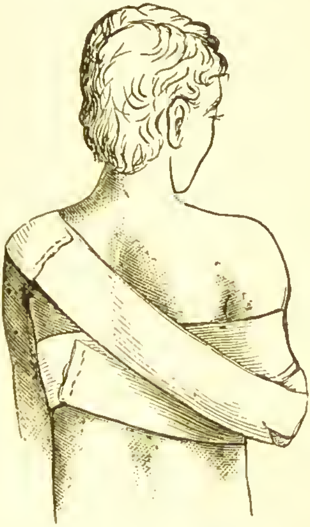


FIG. 10.



strip round everything, to hold all firmly together. I think this is a very good method. It is a modification of Velpeau's old method, which had the great disadvantage that it simply consisted in bringing the elbow up without any fulcrum through which to act upon the shoulder. That is the mistake usually made in putting on Sayre's apparatus—that the upper part of the humerus is not drawn back sufficiently before bringing the elbow forwards. Unless you do that, you do not fulfil all the indications of the apparatus.

Lastly, with regard to the scapula. Fracture of the scapula is an exceedingly uncommon event, and it requires very little treatment. It usually occurs from a crush of the patient. We have an example of the accident here in which the scapula has been broken across. I need hardly say that, being covered by muscles in front and at the back, there can be very little displacement, and if there were you could not overcome it. You may as well, therefore, let it alone, putting on a broad bandage as for fractured ribs. The result will be quite satisfactory.

Then a word as to the length of time that you should keep up fractures, and what you should do with them afterwards. It is not sufficient simply to get fractures united; you must get the patient's limb into a good state. It is a mistake to lose sight of the patient and only to tell him to go about using his arm until it has got all right.

Perhaps it does not get all right, and after six weeks or three months the man goes to the nearest bone-setter, who does what you ought to have done, moves the joint, and he gets all the credit, and the story goes about that you have made a mess of the case and overlooked a dislocation!

If you want to take care of your own reputation, do not dismiss a case of fracture until the patient has complete use of his limb. The fracture may be united, but if the joints are stiff the patient cannot be considered well; you must restore power to the joint by moving it, and if necessary putting the patient under chloroform for the purpose. If you attend to what I have said with regard to the treatment of these several fractures, you will not have so many stiff joints as some surgeons have. The fact is that cases are treated a little injudiciously by splints, and the splints are kept on too long. For ordinary fractures of the upper extremity three weeks or a month is ample time for the splints. You should take off the splints at the end of that time, and let the patient have a sling. When he has had the sling on a fortnight or so, let him begin carefully to use his arm, and probably he will get a much better arm than if you kept the splint on longer. Of course each case must be taken on its own merits. A young healthy patient requires less time than an older and feeble patient. It may be necessary to keep a splint on the upper arm, for instance, over a month; but if you do keep it on, you might take the splint off at the end of three weeks and gently move the joint (it is a great mistake not to do that), and then put the limb back again on the splint, if you do not think it is safe to leave it off.

One word before I close with regard to the difficulty you will all find in treating cases of fracture. Knowing what that difficulty is, I would beg you to be charitable to other people. Do not think that because there has been a bad result it must necessarily be the fault of the surgeon, because it is not so. You will find that with the best advice and the most skilful treatment fractures will sometimes do badly. There must be obscurity in the diagnosis in many cases, and difficulty in carrying out treatment in others. Therefore, although you should make every effort to make a good diagnosis and apply the best treatment you can, you must be prepared for disappointment. And if you come across cases which have been treated by others, be a little charitable about the methods they have adopted.

ON

BURNS AND SCALDS.

YOU will remember that when we went round last Friday week you saw a child which had just been admitted into No. 5 Ward with very bad scalds of the body, and I took the opportunity of pointing out to you that the child had really recovered from the first shock of the injury when we saw it two hours after the accident, and was not in the least collapsed. I proceeded at once to dress the case, but you will remember that, notwithstanding all our care, the patient died two or three days afterwards, and we saw the result of the necropsy this day week. That is only one of a number of cases which come into the hospital during the winter from either burns or scalds, and it is therefore convenient to say a few words to you on the treatment of these cases at the present time.

In the first place, what is the difference between a scald and a burn? They are both, of course, the effects of heat; but a burn is the effect of dry heat—flame; and a scald is the effect of moist heat. But if you have liquid metal, like molten lead or iron, you will find that it produces exactly the same effect as a burn; and some of the most terrible accidents that you will see, if you have to do with mining countries, will be in the cases of patients who accidentally get their feet into molten iron, so that they are charred or burned off, when of course secondary amputation has to be performed.

Then with regard to the mode of treatment of a burn. You may, in private practice particularly, see the cases almost immediately after the burn, and you will find that all these patients suffer from shock, usually in a marked degree. The shock is due not merely to the effect of the burn, but to fright. There is nothing more alarming to women than to find their clothes on fire, and the tendency is to rush screaming out into the street, which is the worst possible thing for them to do. If you ever have to deal with such a case, I would recommend you at once to throw the sufferer down and roll her in a thick rug, a great-coat, or anything of the kind, which will put out the fire immediately. Having thus stamped it out, you can proceed to treat the burn. Of course in a hospital we do not get cases so soon; they are brought here a little after the burn, and generally still more or less in a state of shock.

Let me warn you against treating the local injury instead of the shock. If you treat the local injury, there is an elaborate dressing, with more or less exposure of the person necessarily; you thus increase the shock, and may very likely kill the patient. The proper method is not to treat the local injury at all at first, but to treat the shock by restoring warmth to the body, by wrapping the child in a hot blanket, putting it in front of the fire with hot bottles to its feet, giving a little hot brandy-and-water or something of the kind by the mouth (even an infant will bear a few drops of brandy, particularly while reaction is coming on), and giving a little opium to relieve the pain, for there is no injury more agonising than a severe burn on the skin, and as long as the pain lasts the shock will be kept up. As soon, therefore, as reaction shows itself, it is wise to give laudanum, and you may give it to children, but you should be cautious about the dose. The usual rule is one drop of tincture of opium for every year of the child's age; but you may give more than that if you are careful in breaking up the doses. What I do in the case of these young children generally is to put five drops of laudanum into an ounce of water, and then tell the sister of the ward to see that they have a teaspoonful every hour or two according to the circumstances of the case. Repeating these small half-drop doses is much better than giving larger doses, which may produce dangerous narcotism.

But supposing that the patient does not die from the immediate effect of the accident but rallies from it, and you have to treat the case, what are the indications for treatment? They of course vary according to the severity of the injury. And here we come at once to the classical division, as we may call it, of Dupuytren, usually given in books. I mean those six degrees of severity of burns, which are all very well, but in practice it is quite sufficient to have three divisions. The point for you to determine is this: is it simply a scorch such as you may get from an explosion of gas; or is it something more severe, leading to vesication (which is the condition in which you saw the child the other day, and is the common condition of scalds); or is it something more severe still—a charring which can only occur from the application of dry heat, actually scorching up and burning the part? Those three degrees are practically all that you require. I quite allow that charring may be very different in different cases. You may have a merely superficial burning of the skin, or the muscles may be scorched, and the whole tissues of the limb burnt up and the limb destroyed. Upon those degrees Dupuytren founds three of his divisions, but practically, as I have said, it is quite enough to remember that you may have a superficial burning, or a burning with vesication, or a burning with an eschar, which is the most severe.

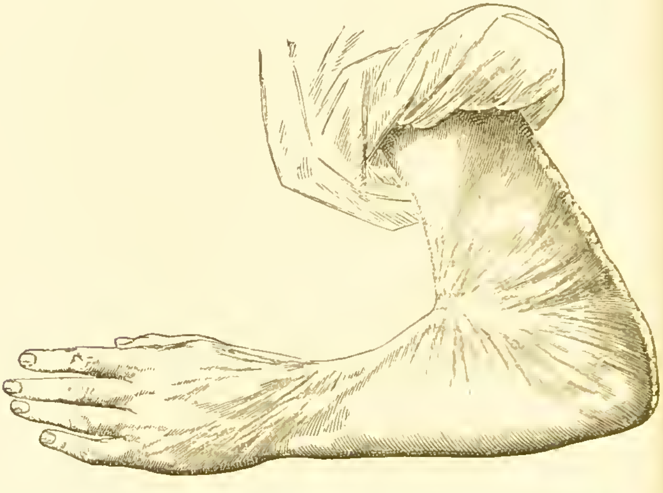
First, then, is the superficial burning, as in the case of a foolish person searching for an escape of gas with a light, and getting an explosion of it in his face. There is a good deal of scorching, and of course a good deal of pain. Perhaps before you have seen the case it has been powdered over with flour, and that is not at all a bad application. But you may see it before that, and if it has not been powdered there are various things which you may apply. Any greasy application is good, but those things are rather inconvenient on the face, and for face burns I think a very convenient application is a mixture of collodion and castor oil. The castor oil supplies the greasy portion, and the collodion mixes well with it. The proportion is generally one of collodion to two of castor oil, to be painted upon the face. It does not set firmly like ordinary collodion, but it sets sufficiently to hold its place and to protect the skin from the air, which is the great point, and at the same time without any injurious or uncomfortable pressure upon the part. Then, again, nitrate of silver painted upon the burn is very good, only it smarts severely for the moment. The late Mr. Skey was very strong in his advocacy of that, and it is not at all a bad remedy if you apply it in suitable cases. In the case of small superficial burns, a ten-grain solution will make a good application, or you may employ what is to be found in a great many houses (at least, used to be)—the nitrate bath of the amateur photographer. Of course, the nitrate turns the part black, and you must prepare the friends for that; but it forms a little superficial eschar all over the burnt surface, and in that way protects it from the air.

Then take a case such as that which we saw last Friday week, where vesication has already been produced. What are these vesications, and what do they contain? It is not merely serum; if it is serum, it has got some fibrinogen in it, for if you leave it the material which is exuded becomes solid, and you may see every now and then in the surgery a patient who has had a scald some days before, and treated it domestically, and who has come here with a mass of coagulated albumen beneath a large bleb of cuticle. Now, it is important not to let that coagulation take place, because it is difficult to treat. The proper plan—the one which you saw me adopt the other day—is to snip the vesicles in a depending position, pat them down, carefully preserving the cuticle on the surface beneath, and then apply carbolic oil. Another very good dressing is the boracic ointment, which has the advantage of being greasy, and is to a certain extent aseptic.

Then we come, lastly, to the case where there are eschars; there we have a burnt surface which is clearly destroyed, and of course time must be given for the slough to come away. Now comes the great point of preserving the patient as far as possible from the risks of pyæmia. No doubt in former times many of these cases of severe burn died in a week or ten days after the injury from pyæmia; that is, they were allowed to lie in a bath of pus of their own formation, and then some deleterious matter contained in it became absorbed, and in that way the patient got pyæmia and died. The great thing is to guard against this by using absorbent dressings, and at the same time dressings which shall be antiseptic as far as possible. Here, again, we may use carbolic oil as a dressing next the surface, and over that plenty of cotton-wool—it may be the salicylic wool—in order to absorb the fluid.

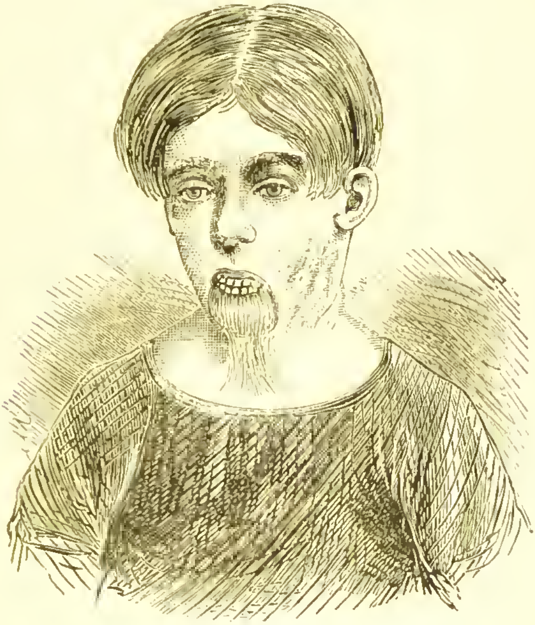
So also in extensive surface burns it is good to dust a little iodoform upon them, or to apply vaseline containing 10 per cent. of iodoform, treating them as you would any other surfaces which you want to keep sweet, and at the same time to get the fluid absorbed. Where sloughs are hanging fire and do not seem likely to separate, poulticing for about twenty-four hours will not do any harm, particularly if you sprinkle a little iodoform on the poultice. When the sloughs have come away, and you can assist them by snipping little bits of dead tissue with the scissors and pulling them away with the forceps, you find beneath a large granulating surface, which may be more or less healthy or may be somewhat flabby, when you can bring it into a healthy condition by stimulating lotions. It is in the treatment of these cases that the greatest trouble is found. An ordinary superficial burn heals without any great difficulty, but when once you have destruction of skin you cannot be too careful in the after-treatment, because, unless you watch the case for weeks and months, you will perhaps find that in the end a terrible cicatrix has formed, which you are blamed for, although it may be from no fault of yours.

FIG. 11.



What commonly happens is this: You have got, we will say, a burn on the arm, and you are taking great pains with it. You are getting away the slough, and are producing a nice granulating surface. You put a splint on the back of the elbow, and you let the surface granulate up gradually and slowly. Then you lose sight of your patient for a time, and after some weeks he comes back with a beautiful web stretching across from the upper to the lower arm (Fig. 11).* He has got tired perhaps of your treatment, and has gone to some old woman or quack, who has said to him,

FIG. 12.



"Leave off your splint, bend the arm, and it will soon get all right." The arm has been bent and the cicatrix has been rapidly formed, and when you see the case again there is a web stretching from one part of the limb to the other, the result being very disastrous to the patient. I have here photographs of cases of that kind; and here is another on the neck (Fig. 12). Those are the most serious cases of all, where the neck has been badly burnt and where you have cicatrices drawing down the lip.

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* Figs. 11 and 12 are taken from Fergusson's *Practical Surgery*, by permission of Messrs. Churchill.

In all those cases where you have extensive burns to deal with, you must have great patience in the treatment. They are cases which remain in the hospital for months together; they wear out the house-surgeon, the dressers, and the nurses; and yet they are worth attending to, because, unless you are careful, they are sure to do you discredit. The great secret is to superintend the granulating wound, and keep it on the stretch while it is healing. You cannot have a better example of this than the one which I mentioned just now of the arm. If you have a granulating surface in front of the arm, there must be a splint put on the back of the elbow, and the patient must wear it for six months. It is the same with the neck; if there is a granulating wound, the result of a burn, you must keep the child's head constantly stretched over a pillow till the wound granulates up; and even then it is well to have some form of apparatus by which you can keep the cicatrix constantly on the stretch for months afterwards; because it is not merely the healing that you have to consider, but it is the after-contraction of the cicatrix which is so serious. You must bear in mind that, although the case may look very nice, if you let it go without attention, in six months it may come back in a terrible state, and you will be blamed for it.

But supposing that, unfortunately, the cicatrix has been allowed to contract and the case comes to you for treatment, a good deal may still be done.

FIG. 13.

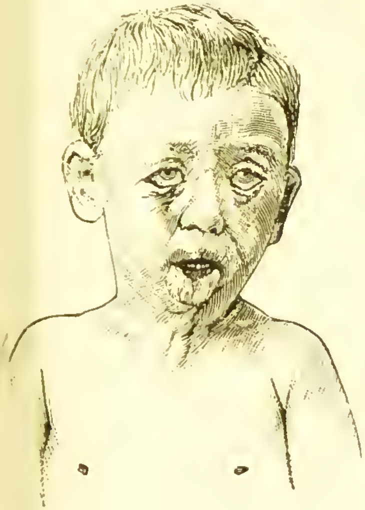
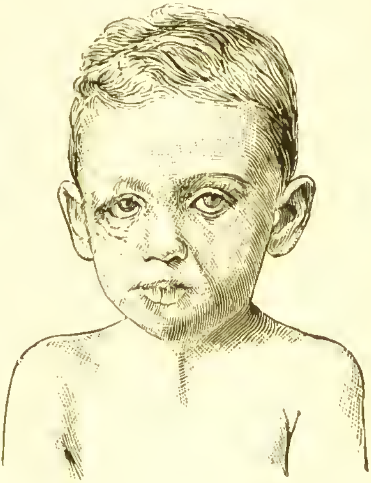


FIG. 14.



Here is an example in a photograph (Fig. 13) of a child in whom the lip was entirely drawn down—turned, as it were, inside out; and here is also a photograph (Fig. 14) of the child taken after treatment. The operation I performed here was that suggested by the late Mr. Teale, of Leeds, and you will find it described in Mr. Erichsen's

Surgery, with a diagram which I will reproduce on the black-board. The everted lip was divided into three parts by two vertical incisions, the central portion being equal to half the width of the lip, and the lateral portions taking each a quarter. From the lower end of each vertical cut an incision was carried to an inch beyond the angle of the mouth, and the two flaps thus marked out were dissected up and brought together in the middle line, being fixed upon and supported by the central portion of the original lip, from which the everted mucous membrane was dissected off. The everted eyelids also were improved by making a V incision below them and dissecting them up so as to convert the V into a Y, with the result of pushing the lids up considerably. In cases of recent burn it is well to make the edges of the lids raw and stitch them together, to prevent if possible the unsightly eversion which is likely to follow.

When you get extensive burns about the neck, the difficulty is how to fix the parts. You have here represented in a photograph a plan which I carried out, and which I think is rather useful. The case was that of a boy who was in the hospital in 1876–77. He had, as you see, a contracted arm and neck. You see also that his head was shaved, and the reason was that I found that the only way to make traction upon the cicatrix was to take strips of plaster up to the top of his head and to fix them to the scalp. The instrument makers make all sorts of apparatus for these cases, but they are all more or less unsatisfactory, and the reason is this—the tendency of them all is simply to pull upon the cicatrix in one direction. What you want, however, is to stretch the cicatrix, which requires a great deal of ingenuity, and the best way to do it is to have a piece of sheet lead fitted to the collar-bones, and another against the chin and lower jaw, and to have screws between the two which will push in opposite directions. If you thus stretch the cicatrix, you will get a good result; but if you merely pull up the cicatrix, the tissues about the shoulder give way, and the patient's last condition will not be much better than the first.

Leaving that part of the subject, I want to say a word or two as to why patients die after they are burnt. In the first place, they die from shock. In the old days of crinoline, which I am afraid is coming in again, cases were not very uncommon of girls, with ball dresses on, putting the candle down on the ground just to see that the last tuck was right, and then catching fire, and being a mass of flames at once. Those patients generally died, the burns being so extensive all over the body that the poor girls died in a few hours after great suffering.

But it is not absolutely necessary that a patient should die from her clothes catching fire. We had a case, you may remember, in the hospital two years ago of a servant girl who, turning round in front of the kitchen fire, set the back of her dress alight. When she came here, she was frightfully burnt all over the back and the buttocks, but, fortunately, not in front, since a burn over the front of the body is much more serious than over the back. She had to lie on her face for many weeks, and we got the sloughs away, and the parts healed nicely. Fortunately, the cleft of the buttocks had not been affected, so that the rectum and the vagina were not interfered with, and the result was most satisfactory, the patient leaving perfectly well.

But where patients do not die of shock, they are apt to die, as the child did, of an affection of the lungs. They either get bronchitis or some low form of pneumonia, and die from that; or they may get diarrhœa and pass a little blood, and then suddenly die; and at the necropsy you find that there has been a curious condition of ulcer of the duodenum, which the late Mr. Curling of the London Hospital first called attention to as the result of burns.

I cannot tell you why it is the duodenum particularly that ulcerates; nobody seems to know. There are two or three theories about it, but I do not think that anybody really knows. There is no question, however, about the fact that ulceration of the duodenum is apt to take place, and then perforation or possibly hæmorrhage from the mesenteric artery, and the patient dies from that.

It is important, then, not to set up diarrhœa in these cases—as important in the case of burn as in a case of typhoid. Nobody in his senses would think of giving purgative medicine in a case of typhoid, for fear of diarrhœa and ulceration of the bowels; and it is the same in the case of burns. You want to feed the patients well, to give them easily digested food, and if the bowels do not act you can treat them sufficiently by enemata acting on the large intestine, but letting the small intestines alone.

Then of course you may have deaths from other causes. Patients die occasionally with cerebral symptoms—from congestion of the brain, and so on.

Before I close I want to say a word on one form of scald—the so-called "scald of the glottis," though I would rather call it "scald of the larynx," because that is what it really is. It happens generally in this way. Poor children are left in a room by themselves for many hours, and, getting exceedingly thirsty, they want to drink something, and they go to the kettle, which is boiling on the fire, and they get a gulp of steam or hot water, instead of the cold water they wished for. The result is that a child gets a scalded mouth. But no further notice is taken until in the evening it is observed that the child begins to breathe with great difficulty, and then possibly medical advice is sought. What happens is this: The scald affects the upper part of the air-passages, and after a few hours, not immediately, as the result of the application of hot steam, œdema comes on, and it is the œdema supervening a few hours afterwards which is so dangerous in these cases.

In the museum of the College of Surgeons you will see a very good specimen of œdema of the larynx. It shows how all the ordinary sharp outlines of the larynx become obscured by the elevation of the mucous membrane due to the œdema beneath. The symptoms are unequivocal. The child is gasping for breath as in a case of croup, and you find that the mouth is scalded. The first thing to be done in such a case is to enclose the child in an atmosphere of warm steam, to erect a tent over the bed, and to bring steam underneath it from a bronchitis kettle. But if the case is severe, that is not enough; you must at once proceed to scarify the throat. That is a simple operation, and an exceedingly useful one, and I believe if it were more frequently done early we should not have so many deaths from scalding of the larynx. You take a sharp-pointed bistoury and cover the heel of it with a strip of plaster. You then put your forefinger on the back of the bistoury so as to cover the point, and you slip your finger down the tongue until you come to the swollen epiglottis, which stands up and is three times its proper size. Then with the point of the instrument projecting beyond your finger you scarify pretty freely about the root of the tongue, the epiglottis, and the top of the larynx, where I do not think you could do any harm.

People are afraid of undertaking this simple operation because they do not know exactly where the carotid artery is! If they do not, I am sorry for them; but certainly it is nowhere near the epiglottis, any more than it is near a tonsil which they may be going to remove. I never knew any hæmorrhage occur from puncture of œdema of the larynx, and you may incise and puncture it with the greatest relief to the patient. Then you give some hot milk-and-water to gargle the throat with; and a hot linseed poultice put under the jaw would give the patient considerable relief.

Still, the question may very likely arise as to giving relief by opening the windpipe. You cannot have a better test of the necessity for interference than what is called "retraction of the chest." You see the child taking a breath, and when it struggles in doing so, instead of the ribs rising, they fall in, very little air entering the larynx, and retraction of the chest-wall taking place. Whether that is due to diphtheria or croup or a scald, it does not matter; it is a most important symptom, and if you see it you ought to open the windpipe at once. Where will you open the windpipe? Undoubtedly at the crico-thyroid membrane. There seems to be a prejudice in some surgeons' minds against doing laryngotomy in children. I cannot see why; it is a simple operation, for you can feel the cricoid cartilage distinctly enough to be a guide, and nothing can be simpler than to make an incision through the membrane and put a tube in. Laryngotomy is quite sufficient, because, if you look at the anatomy of the parts, you will find that the œdema never goes below the true vocal cords—that was settled years ago—and you are well below the vocal cords when you do laryngotomy. The reason why I urge the operation in this case of scald, as well as in the case of a foreign body, is that anybody can do it with a penknife. If a man has not done tracheotomy on the living body, it is very likely that, having heard about the dangers of the operation, he may possibly fight shy of it, but he cannot well do so with laryngotomy, because he cannot go wrong; and therefore I advise it in these cases.

Then as to drugs. If you have time you may try the effect of antimony, which I have more faith in than anything else. Give full doses of antimony according to the age of the patient, and be ready to do laryngotomy if required. Do not put it off too long. You will find that if laryngotomy is performed early in these cases you will not have to reproach yourself afterwards with having allowed the patient to die unrelieved.

ON

ANEURYSM.

We have now in the wards a case of popliteal aneurysm which is especially interesting as occurring in a woman. Aneurysm in a woman is comparatively rare, and certainly popliteal aneurysm is very uncommon, for I do not myself remember to have seen one before. The reason of it in the present case is easily to be found, and you will remember that I called attention to it when the patient was first admitted. She was paralysed on the right side, and the explanation therefore is that the left leg has had to do more than its share of work, and consequently the artery has become aneurysmal. This woman is forty-seven years of age. There is nothing special in her history bearing upon the disease, and I need not trouble you with the details. She seems to have noticed a lump in the popliteal space for some little time since Christmas, and she came here one day complaining of something the matter with her knee. You will remember that I did not see her with you till Friday week, and we then found that she had a well-marked popliteal aneurysm.

Let me say here one word with regard to the diagnosis. There was a pulsating swelling which was evidently in direct communication with the artery, because the moment we compressed the femoral artery we stopped the pulsation in the sac; then, on very gently pressing the sac while the artery was held, we found we could slightly diminish it, and on letting go the main artery again we got the distensile pulsation which is so characteristic of aneurysm. The whole of the pulsations are more or less distensile, for if you put your fingers upon the aneurysm you feel that they are separated one from the other; but the best way of demonstrating it absolutely to a class is to stop the pulsation for a moment, and then allow the tumour to swell out suddenly; you then see at once that it must be an aneurysm.

There was in this case an aneurysmal bruit, which means of course that on listening to the sac we hear the sound of the rush of blood. You must not suppose that every aneurysm has a bruit, nor, on the other hand, that every tumour which has a bruit is an aneurysm. You can produce a bruit artificially in a healthy artery by pressure with the edge of the stethoscope, and some cases of vascular tumour have a very distinct arterial bruit in them. Sometimes you get quite a noisy bruit, as in cases of so-called aneurysm of the internal carotid in the head, but in many of these there is no aneurysm. We found that the woman had a large saccular aneurysm. Most popliteal aneurysms are sacculated, for it is very rarely indeed, I believe, that you get a fusiform aneurysm, which is an aneurysm formed by a uniform dilatation of the coats of the artery.

With regard to the division into true and false aneurysm, I should hold that every aneurysm which still contains blood within the coats of the artery is a true aneurysm ; but if the coats of the artery have given way, and the blood is outside the artery, and a sac is formed by the surrounding tissues, I should call that a false aneurysm. I am aware that that is not strictly the definition always given, but it is sufficient for all practical purposes. And now as to treatment.

I thought that both for the woman and also for you it would be well to try other methods of treatment before resorting to the *ultima ratio* of ligature of the artery. There did not seem any special urgency about the case to require the ligature immediately. Therefore you will remember that we arranged that there should be digital compression of the main artery, and we told off the dressers *seriatim* to hold the common femoral artery and cause stagnation of the blood in the sac. What would be the method of cure brought about by that treatment? It is obvious that our object was to stagnate the blood in the aneurysm, and thereby, if possible, to produce coagulation of the contents of the sac. If we had succeeded—which we did not do—the clot formed there would have been very much like the clot formed in the basin when you bleed a patient; it is the clot sometimes called a "soft clot," which was held by the older authorities on aneurysm—Broca particularly—not to be capable of producing a true cure of aneurysm. I quite agree that in many cases that clot undergoes changes afterwards which lead to an unfortunate result. I mean that you may get a certain amount of clot, as we did in this patient, by careful compression of the main artery, and a very few hours after the compression is left off the whole of the clot is washed away into the general circulation, and the aneurysm is beating much as before.

Now, the pressure was kept up for four hours on February 22nd, from ten minutes past three until ten minutes past seven, on the very first day on which we saw the patient. At the end of that time the tumour seemed distinctly harder and the pulsation less, no movement of the knee being caused by the pulsation. But the next morning when I saw her I thought that there was not any definite improvement; in fact, the little clot that was there had been washed away.

I therefore adopted a plan of treatment which is sometimes useful in these cases—namely, flexing the joint forcibly, bending the knee so thoroughly that the aneurysm is slightly compressed against its own artery. In that way we found that we could very nearly stop the pulsation, not completely, and that position was kept up during the whole of the next day. But there was no permanent improvement, and on February 24th instrumental compression was begun at 10.45 A.M., and carried on till 8.45 P.M. A Carte's tourniquet was applied to the groin, and a horseshoe tourniquet below it. The pulsation of the aneurysm was not completely arrested purposely, because we did not want in that case completely to stop the flow of blood.

Let us now contrast these methods. The object of the first, compressing by digital means, and stopping pulsation in the aneurysm completely, was, if possible, to produce coagulation of the contents, and the flexion treatment had exactly the same object. But the instrumental compression which we took up on February 24th had a very different object; we purposely did not stop the circulation completely through the aneurysm, but we allowed a little blood to trickle past by just graduating the compression sufficiently to obstruct but not to stop the artery. Under these circumstances we hoped to get a cure by quite a different method—namely, a deposit of laminated fibrine within the sac, and this method is no doubt that by which nature cures aneurysms. You see occasionally in post-mortem examinations an aortic aneurysm, for example, either absolutely cured, or so far cured that it gives no trouble, by thick deposits of laminated fibrine. If you look in the museum you will see plenty of specimens illustrating this point, and you may see sections of cured aneurysms in the bottles on the table. You will observe dense white fibrine which must have been deposited in layers one upon the other from the outside of the aneurysm towards the centre. That is the method of cure by laminated fibrine, and it is no doubt the best method; indeed, the older surgeons held that it was the only cure, but that is not true. Aneurysms can be cured by coagulating the contents of the sac, but the cure is apt to be evanescent.

I ought to have mentioned that I put the patient first upon a limited diet—namely, meat, six ounces; bread, six ounces; milk, one pint; tea, half a pint; and two or three ounces of rice, to be made up into a pudding with a little milk. She had, you observe, a full allowance of meat, but I limited the bread and cut down the fluids as much as possible. That is the kind of diet which you want to give to aneurysmal patients. You want to increase the plasticity of the blood, and you do that by diminishing the watery part of it, putting the patient upon a limited amount of fluid, and also increasing the fibrinous portion of the food. Then I put her upon iodide of potassium in twenty-grain doses. There is a feeling among physicians rather than among surgeons that iodide of potassium does good in some cases of aneurysm. It certainly could do no harm, and therefore I thought it well to give iodide of potassium at the same time that she was under these various methods of treatment. Nevertheless we failed.

Was there any other method which might have been adopted before I tied her artery? There was one, but I did not think it well to adopt it; I think it right, however, that you should know it. It is the application of Esmarch's bandage. In order to do this properly you must put on an elastic bandage from the foot up to the aneurysm, and you must put on another from the thigh downwards to the aneurysm; the result being that you squeeze all the blood of the limb into the aneurysm so that you make it tense, and you make the blood perfectly stagnant in the sac. If you can do that for two or three hours you may hope to produce coagulation of the contents. Some years ago, when the plan was first suggested by Dr. Reid, R.N., I tried it here in two cases, and I have brought the notes of them with me.* The first case was that of a man, aged thirty-two, a coal miner, who had a large popliteal aneurysm. On October 29th, 1879, at three P.M., half a grain of morphia having been injected beneath the skin of the right thigh, Esmarch's bandage was applied to the limb in the usual manner, reaching to the lower border of the sac. A piece of Martin's india-rubber bandage was applied from above downwards in the thigh, stopping well above the aneurysm, and over the bandage the elastic cord was tightly drawn. At four P.M. the patient was placed under the influence of dichloride of ethedene (now we should put the patient under ether), the pain produced by the treatment having become very severe. At six P.M. the tourniquet and the bandage were removed (the former first) and the anæsthetic discontinued; all pulsation had ceased in the sac, and it was apparently solid. One hour afterwards, however, a slight return of pulsation was noticed by the house-surgeon. Two of Carte's compressors were therefore applied (one to the groin and the other to the middle of the thigh), and the artery was controlled by them alternately. Compression was thus kept up until nine A.M. on the 30th. The result was a complete cure.

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* *Clin. Soc. Trans.*, vol. xiii.

The second case was that of a man, aged thirty-seven, who was admitted under my care on December 2nd, 1879. He had a large popliteal aneurysm which was thought to be fusiform. The aneurysm had no thrill in it, and no bruit.

On December 3rd Esmarch's bandage and tourniquet were applied as before, and were kept on for three hours and a half, the patient being under the influence of dichloride of ethedene the greater part of the time. He also made a good recovery.

You may naturally say: "If those two patients recovered, why not adopt the method in the case of this patient?" Well, because there is, no doubt, a little risk of gangrene, and other surgeons have not been so fortunate as I was in those cases. I therefore preferred to tie the femoral artery.

Having made up my mind to tie the artery, I gave the patient (all these methods being a little exhausting) a couple of days' rest, and on February 27th I tied the femoral artery—of course the superficial femoral artery; I tied it where it is crossed by the sartorius muscle, and there was no difficulty in finding it. I made a free incision (and I am sure, if you are to tie arteries successfully, you must have plenty of room) through the skin and fascia, and exposed the sartorius muscle, which was drawn a little outwards. Then the sheath of the vessel was exposed, which I opened carefully with a knife and forceps, and then slipped the needle round, disturbing the parts as little as possible.

I feel confident that a successful result depends very much upon the way in which the artery is tied. If you isolate it much, if you pull it about and bruise it with directors and instruments of that kind, you can hardly expect that the artery will do well afterwards. I am sometimes shocked at examinations to see young surgeons, when called upon to operate, taking a steel director and tearing out the artery for an inch or more in length. Luckily it is a dead artery, so that it does not much matter; but if it were living, the chances are that there would be secondary hæmorrhage. I think that is one explanation of the statement lately made by Mr. Ballance at the Royal College of Surgeons that the great mortality after the ligaturing of large arteries was due to secondary hæmorrhage. Now, I have tied a pretty good number in my time, but I am happy to say that I have never seen secondary hæmorrhage. I saw Sir W. Fergusson's practice for many years, and he tied a great many arteries, but I never knew a case of secondary hæmorrhage; but I have known it occur in the practice of another surgeon in two cases. One was in the femoral and one in the carotid, and both cases were fatal, as such cases usually are. The artery may be too diseased to bear a ligature. That, of course, is very unfortunate; and if you do get an artery so diseased, perhaps the best course is to use a very large ligature. But if the artery is healthy enough to bear a ligature, it is possible that it may have been too much torn or disturbed at the time of the operation.

That is one great argument against a practice which is a very old one, but which has been revived within the last few years at St. Bartholomew's Hospital—viz., tying the artery in two places and dividing between. The theory is that, if you have an artery divided in that way, its elasticity is expended by retraction, and there can be no tension upon the point of ligature. Now, this practice seems to me to add considerable risk to the operation. You must isolate the artery considerably to get two ligatures round it; and, moreover, by cutting the artery through, if one ligature should happen to slip, you lay yourself open to secondary hæmorrhage, and that is what has occurred. A surgeon tied the subclavian artery—you know how short that is—in two places, and divided the vessel between; half an hour afterwards one of the ligatures slipped, the result being that excessive hæmorrhage occurred, and the patient died.

My advice is to tie with a good silk ligature and to tie tightly, and that brings me to another matter which has been lately laid down by Mr. Ballance in his lectures—namely, that we are wrong in tying tightly, and that we should not tie sufficiently tightly to divide the coats of the artery. Let me explain what you do when you tie a ligature tightly. What you do is to divide the internal and middle coats, which contract and retract above and below the ligature, by which the external cellular coat is firmly embraced. That I believe is the best plan, because if you use, as we now do, an aseptic ligature, nothing more happens. The ligature in the case upstairs has never come away, and never will; it is lying quietly, surrounding the external coat, and strengthening it materially; the middle coat has retracted and become agglutinated by lymph, and firmly closed; and therefore the risk of secondary hæmorrhage is reduced to a minimum.

I quite allow that when we had septic ligatures, when we used to tie with silk or hemp, which came away afterwards (and we left the end of it out to pull at), there was a little more risk. Supposing that firm union had not taken place, and the parts had not become amalgamated, then was the time for secondary hæmorrhage to occur. I have here a specimen which illustrates the subject. Here is the carotid of a young woman who died some years ago. I had applied a ligature, and the patient died a few days afterwards from softening of the brain, which is not a very unusual result of ligature of the carotid. The cut ends of the aseptic silk are still to be seen in position. The coats of the artery are evidently tied closely, and you can see a considerable clot above the ligature and a smaller clot below. You could not have a better illustration of the use of an aseptic silk ligature.

You may know, perhaps, that it has been

proposed to use catgut ligatures instead of silk, and when those ligatures first came in I tied a femoral artery with one of them in this theatre. It was a case of popliteal aneurysm, and I tied the femoral in the ordinary way; but in a few days pulsation returned in the aneurysm and all the way up the femoral artery. It was evident that in the effort made not to tie the artery so tightly as to cut the coats, I had not obliterated the artery at all; and that the catgut ligature, becoming softened, had given way, and so the current of blood had poured on. I solved the difficulty by putting a silk ligature a little lower down, and the patient got well.* Mr. Thomas Smith, of St. Bartholomew's, was not so lucky. He had a case of that kind, where he tied the femoral artery with catgut, and the same thing took place; the catgut gave way, and the pulsation returned. But, unfortunately, he tied the ligature a little tighter than I did, and the result was that he weakened the coats of the artery at the point of ligature, and within a few weeks an aneurysm developed at that point, so that the patient's condition was aggravated by the operation, because he had a second aneurysm developed at the seat of the ligature in addition to the one below.

So much for the question of ligature and the question of operation in this case. Now let me say a word with regard to what has happened in the sac of the aneurysm since the ligature was tied. Is it a cure by coagulation of the contents of the sac, or is it a cure by deposit of fibrine? It is a little difficult to be sure in this case, because I am happy to say we have not had an opportunity of examining the sac, and I hope we shall not have it, but I rather think it is a cure by coagulation of the contents, and for this reason. We had a certain amount of clot in the sac before the operation. I then tied the femoral artery, and we have never had the slightest pulsation in the sac since. Now, in order to get deposit of laminated fibrine you must get a current of blood going through the sac, but we have never had anything of the kind, it has been solid from the first; therefore I think I may say (with a certain amount of hesitation) that we have here a cure by means of the coagulation of the contents of the sac. The effect of the ligature was to do what we tried to do by digital compression, but we failed then to get complete stasis of the contents of the sac, and as a consequence of the ligature we have now coagulation of the contents of the sac and a cure. Therefore, believing that to be the case, I have been very careful to avoid manipulating the sac since. I think it is quite possible that if we were to over-manipulate the sac and pull it about too much we might displace some of the clot and disturb the cure.

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* *Clin. Soc. Trans.*, vol. xi.

With all your care accidents will sometimes happen, whether the cure is from coagulation or from deposit of fibrine. Sometimes the sac of an aneurysm gives way within a very few hours of the ligature of the main artery, and it is a serious matter, because it almost always involves gangrene of the limb and amputation. What is the explanation? I believe it is this, that there is a very thin sac which has possibly undergone a certain amount of manipulation in previous attempts at cure by compression. Then the surgeon resolves to tie the main artery, and when he is doing it some of the bystanders, being very anxious to see that the pulsation has ceased, manipulate the sac. I always take care to avoid that. I only allow one gentleman to put his finger on the sac to assure me that the pulsation has ceased.

Then there is another thing that may happen, and it might happen in this case—inflammation of the parts surrounding the sac might come on. It is said to be inflammation of the sac itself, and possibly it may be; but I think it is rather of the surrounding structures, and is set up probably from the same causes as those I have mentioned—viz., by over-manipulation, and then an abscess forms and has to be opened, and you find that it communicates with the sac of the aneurysm. Now, that is not necessarily a fatal thing at all. If it does not occur for some days after the ligature, the aneurysm has become solid, as you see in these specimens. You open the abscess, and perhaps some of the clot may be discharged, and perhaps not; and although it is an anxious case, still the patient may do well. I have known it occur in a case of carotid aneurysm. A colleague of mine tied the carotid, and subsequently suppuration of the sac of the aneurysm took place, yet there was no secondary hæmorrhage, and the patient got well.

I have been hitherto speaking of the treatment of aneurysm entirely by proximal methods. All the methods I have mentioned have been between the aneurysm and the centre of the circulation, but you must understand that there are methods which we are sometimes obliged to adopt called the distal methods, and I will say a word or two about them so as to put the matter clearly before you. There are of course arteries like the innominate, or the first part of the subclavian, or the carotid low down, where you cannot get to the proximal side of the artery. There have been one or two attempts (you may see Liston's specimen in our museum) to tie the first part of the subclavian artery, and to tie the innominate, but they have always proved fatal, because when the ligatures came away secondary hæmorrhage occurred. Liston tried to avoid that by tying the carotid as well as the first part of the subclavian in the case in the museum, but the same thing happened, and the patient died.

Of course it is possible nowadays, with septic ligatures, that we might succeed in getting a ligature on so near the heart, but he would be a bold surgeon who would do it, because it is a difficult operation, and the results have been so exceedingly unsatisfactory. Therefore, for these cases one of the methods suggested for treatment has been what is called the distal ligature, and it is often called Brasdor's operation—not that Brasdor did it. Although it was suggested by a French surgeon, it was an English surgeon, Wardrop, who in 1827 first tied a distal ligature at the root of the neck. He tied the carotid for aneurysm of the innominate, and afterwards the subclavian. You will observe the difference between the two if you think of the anatomy of the parts. The carotid artery runs up from the root of the neck, and you have a considerable length of vessel upon which you can put a ligature without any branch intervening between it and the main artery below. But it is very different with regard to the subclavian. From the first part the vertebral, the internal mammary, and the thyroid axis come off, and from the second part there is the superior intercostal given off.

Now, imagine that you do as Wardrop did—put a ligature upon the third part of the artery. There are a number of branches intervening between the point of the ligature and the innominate artery, so that, as I understand it, Brasdor and Wardrop are not exactly the same thing, though we generally call both the distal operation. Then, of course, you can combine the two operations. I myself, in 1865, tied these two arteries—the subclavian in the third part, and the carotid artery in the neck—in the case of a woman who had a large aneurysm believed to be of the innominate; but it turned out four years and a half afterwards, when she died, that the diagnosis was incorrect, and that it was really a case of aortic aneurysm. Still, she was immensely relieved, and if she had been careful of her health she might have lived longer, I think. The specimen is in the museum of the College of Surgeons. It is beautifully dissected, and it is injected so that it shows all the anastomoses very nicely.

Of course, these operations must be done upon a different principle from the proximal operations, and surgeons are not quite agreed as to what the effect of tying the distal ligature is. Some say, as Mr. Erichsen holds, that by tying the carotid and the third part of the subclavian you cut off two-thirds of the blood, and leave only one-third to come through the arteries. But the fact that you have tied the artery in these two places, and hindered the blood going beyond, does not prevent the normal quantity coming in below; therefore, it would rather seem, if it were followed out, that by stopping the current of blood beyond the aneurysm you would throw a greater strain upon the aneurysm itself, which, we hope, is not the fact. Then Mr. Holmes has suggested that the clotting extends from the sites of the ligatures backwards into the sac of the aneurysm. But I have proved that not to be the fact, and specimens in the museums show that it is not so. Therefore, we come to my own theory upon the subject—I only advance it tentatively—that is, that the ligature applied on the distal side of an aneurysm has some effect in altering the direction of the current of blood. I do not know how, but I am sure that if you tie the arteries beyond an aneurysm it is that alteration in the direction of current which leads to clotting in the sac. I allow that it is not a satisfactory explanation, but I think it is more satisfactory than the other two.

It has been suggested that you should inject fluids into the aneurysm, and this has been tried. Perchloride of iron has been injected. Again, it has been suggested that either horsehair or thin wire should be put in the aneurysm, so as to produce coagulation of the contents, and so bring about a cure. The late Mr. Moore, of the Middlesex Hospital, was the first to do this, and it has been repeated a few times, and with success in the case of the abdominal aorta; but it is a fact that no patient has recovered who has had any foreign body put into a large aneurysm of the root of the neck. Mr. Beck had a case here a few years ago in which he did it, and produced a certain amount of coagulation, but without any permanent effect. With regard to other methods, it has been suggested that injecting ergotine into the neighbourhood of an aneurysm might do good; but it is absurd. The idea is that it will cause contraction of the muscular fibre, but if you look at any specimen of aneurysm you will see that there is very little muscular fibre in it, and it could not be affected by the small quantity of ergotine thrown in.

Lastly, there are two suggestions of Sir W. Fergusson's which I ought to mention. One is, that in the case of an aneurysm of the subclavian artery in the first part, which cannot possibly be treated by a proximal ligature, you should amputate at the shoulder, and then tie the axillary artery at the end of the stump. In that way you get rid of the limb, and therefore, to a certain extent, get rid of the danger of secondary hæmorrhage. Mr. Luther Holden and the late Professor Spence of Edinburgh each had a case, and I myself have had a case where I thought it right (the man's arm being perfectly useless and paralysed) to amputate at the shoulder-joint; but I should not do it again, because I do not think it did any good. The aneurysm progressed as before.

Then there is the question of displacing some of the clot. Sir William Fergusson proposed to manipulate some of the clot out of the aneurysm with a view of blocking the artery beyond. It was done in one or two cases, but without any permanently good result, and there is always the risk of blocking some of the cerebral arteries and thus producing paralysis and death.

ON

STRANGULATED HERNIA.

Last Sunday week we had a case of strangulated hernia. Unfortunately, it occurred at a time when very few of you were here, and the case only lasted a few hours, but I had the opportunity of showing you the post-mortem specimens. The case was this. The man was fifty-six years of age, and he had had a hernia twenty years. On December 6th his bowels acted. On the following day they did not act and the hernia became irreducible. On the 8th vomiting began, and in the evening of that day it began to be stercoraceous. On the 9th he applied to the hospital for relief. So that you see from the 6th, or some time after the 6th, till the 9th, we may say for three days practically, he had strangulated hernia.

The treatment, I am sorry to say, consisted in taking purgative medicines on his own account, and he did not appear to have seen a doctor till he came here. The house-surgeon attempted to reduce the hernia, but, failing to do so, he sent for me. I happened to be out of the way, and Mr. Pollard came and acted for me. He operated on the patient and reduced the hernia. At the same time he performed a radical cure by removing the sac, which you see here, and, as it turned out to be a congenital hernia with a very much atrophied testicle, he thought it wiser, and I have no doubt he was right, to remove the testicle at the same time. That gave him an opportunity of stitching up the canal and performing a radical cure. But, unfortunately, as soon as the bowel was reduced the purgatives which the man had already got in him began to act. He had profuse diarrhœa, and considerable exhaustion following upon that, and he died three hours after the operation.

Leaving this case for a moment, let us turn to the subject of strangulated hernia, which is a most important one. In the first place, what do we mean by it? We mean that a piece of bowel has slipped through one of the apertures through which the bowel is apt to pass, and has become so tightly constricted that not merely is it irreducible, but in addition to that the circulation of the bowel is interfered with. It is very important that you should clearly understand the difference between an irreducible hernia and a strangulated hernia. A person may go for many years with irreducible hernia and no harm may come of it; but, if it becomes strangulated, it is obvious that a very few hours will produce such changes in the bowel that, unless it is promptly relieved, the patient will die of its mortification.

I can illustrate pretty well what the condition is in this way. Supposing that I tie a piece of string round my finger thus, pretty tightly—of course I immediately produce venous congestion, because the effect of the string is to stop the return current of blood in the veins. If I go on and keep the string sufficiently tight and sufficiently long upon the finger, I shall eventually get stasis of the blood in the vein, and then stoppage of the blood in the artery, and finally I shall have the finger mortifying and dropping off. That is precisely what occurs with a piece of intestine which is strangulated. If you think for a moment how extremely tight the ring sometimes is, particularly the femoral ring, you will see at once that the strangulation may be very acute, and you may possibly have an opportunity of seeing, as I have seen, a patient in whom strangulation has only lasted a couple of hours or so, apparently at the point of death.

The explanation of that is not so much the interference with the circulation (because two hours' strangulation is not sufficient to produce any such change in the bowel as will lead to death), but is the effect produced upon the system through the sympathetic nerve, which, being tightly nipped, reacts upon the patient, who looks as if he were moribund within a couple of hours of the strangulation. There was nothing of that kind here. The patient went three days with his strangulated hernia, and when he was operated on the bowel was not in a very bad condition, because Mr. Pollard thought himself justified—and he was justified by the result—in returning it into the abdomen. Here is the piece of intestine which was strangulated. You will see from its appearance (of course it has lost its colour) that it has evidently been in a sac more than once. You will observe that there is a good deal of thickening about it, and it is obvious that there has been from time to time a good deal of action about the bowel, congestion, and some inflammation, which have led to the condition which you see.

With regard to the symptoms, supposing a patient has had hernia for some years, what he generally finds is this. On some occasion, when he has made some extra effort, when he has had, perhaps, a straining at stool, or something of the kind, the hernia comes down and he cannot put it back as he has been accustomed to do. And here let me say that patients are often better able to put a hernia back than the surgeon, however skilful; they have been accustomed to do it, and they have also the advantage of feeling the hernia while they are manipulating it with the fingers. They will lie down and put themselves sometimes in very curious positions, and by kneading and carefully manipulating they will put it back. On this occasion the patient finds that with all his efforts, and trying all that he knows, he cannot put the rupture back, and in such a case, if he were wise, he would take advice at once. But unhappily patients are not always wise, and our unfortunate man, instead of going to a surgeon to overcome the obstruction, took purgative medicine, which was really the cause of his death.

Then, in addition to those cases where a patient has had a hernia, you must remember that a hernia may sometimes be brought down and strangulated then and there. A woman may be in a great crowd, sight-seeing—I have known it occur more than once—and she gets very much squeezed, and afterwards she finds a small lump in the groin, which is then and there strangulated. I remember a case of that kind in 1856 at the Peace illuminations, and the same thing occurred in a patient whom I saw during the last Jubilee year. In such a case the patients are often not aware that there is a lump at all. What they find is that they get sickness, which increases till it becomes stercoraceous, and then perhaps they send for a doctor for what they call bilious vomiting. The same thing occurs in elderly patients, who have felt, perhaps, a little lump in the groin for years and have paid no attention to it until it becomes strangulated. Now, I want particularly to warn you when you get into practice never to treat a case of "bilious vomiting" without being quite certain that it is not a case of strangulated hernia. And do not be content with the word of the patients. They do not always understand what you mean; they may say that they have a lump or they may not; often they will deny that they have a lump because it is so small that they are not aware of its presence. Therefore you ought to make it a rule to put your fingers into the groin and feel the femoral and inguinal regions, particularly the femoral in women and the inguinal in men. And then do not forget the umbilicus, because umbilical hernia is not so uncommon, after all, in women who have had large families, and occasionally you meet with it in men.

In the great majority of cases of strangulated hernia you endeavour, if possible, to put the bowel back. But if you happen to see a patient who is completely collapsed and faint, with a cold sweat on the skin, and apparently moribund, and a history of three or four days' presence of strangulated hernia, you may be pretty sure that the bowel must have got to a state in which it would be very unsafe to put it back, and you had better operate without applying the taxis. But that is an exceptional thing. I suppose almost every surgeon when he first sees a hernia, makes an attempt to put it back. In applying the taxis you should use steady pressure, but no force. If you use force you may do a great deal of harm to the bowel; you may even tear up the whole sac and push it before you, returning it, as we say, *en bloc* within the abdominal cavity, but not relieving the strangulation. But most particularly you should be careful not to burst the bowel. In such a case as this, for instance, where the strangulation had existed for three days, I can easily imagine that if any undue force had been offered the bowel might have been ruptured.

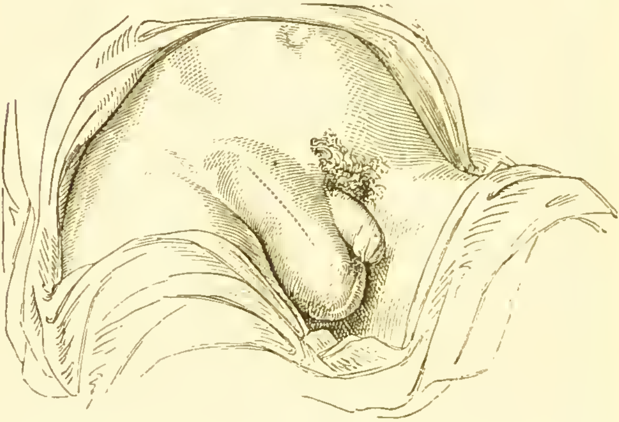
Failing to return the bowel under ordinary manipulation, what is the next best thing to be done? Undoubtedly, to operate. Then, should you do anything in the interval, while making preparations to operate? Many surgeons say, "Put on an ice-bag"; but I cannot imagine why an ice-bag should be put upon a strangulated hernia, because it certainly cannot do any good. I know that there are some authorities for it, but I certainly do not recommend the practice to you. I think you had much better not apply ice, because cold may just act as the turning-point in a piece of bowel which is almost gangrenous, but not quite so. The only thing to be done when you have fairly tried the taxis (if you like, under chloroform) is to operate, and I want you to understand that the operation for strangulated hernia is not a difficult one. Men have been talked to so much about the anatomy of hernia that they sometimes have an idea that hernia is a mystery which they had better not meddle with; and the result is that surgeons, when young and getting into practice, are a little afraid of strangulated hernia, and a good many lives, I believe, are sacrificed because the surgeon does not at once cut down upon it and treat it as he would any other thing that must be relieved, the operation being one which can readily be done by any qualified surgeon.

I need not say a word about the anatomy of hernia, except to remind you where it comes through. An inguinal hernia comes through the internal abdominal ring into the canal, and out of the external ring. Or, if it is a direct hernia, it will burst through the conjoined tendon, and and then come out of the external ring. A femoral hernia would come through the femoral ring (bounded in front by Poupart's ligament, behind by the pubes, internally by Gimbernat's ligament, and externally by the vein), and then would push through the saphenous opening in the fascia lata. That is all the anatomy you want. As to all those minute subdivisions of fascia, particularly the absurd coverings of hernia, they may be forgotten as soon as you like.

Then what is the operation? You have to

cut down on to the sac of the hernia, you have to expose the sac, and probably to open it, but not, perhaps, in every case. In an inguinal hernia, such as this was, you want to get down to the external abdominal ring as nearly as you can, therefore you pick up the skin over the ring, making a ridge at right angles to Poupart's ligament, and then transfix it with your knife.

FIG. 15.



The result is that you have an incision about a couple of inches long through the fat just over the external abdominal ring (Fig. 15).* In femoral hernia you make your incision either over the sac, or, as I prefer it, on the inner side of the sac (Fig. 16). A vertical cut in that position takes you down at once to the femoral ring. In either case—this is the point I wish to insist upon—all you have to do is to cut and tear through the tissues until you come upon the sac. In inguinal hernia you will recognise in all probability one tissue, and that is the cremaster muscle, which you divide upon a director, and beneath which, by tearing through a little cellular tissue, you will come upon the sac. In femoral hernia there is no tissue to be recognised, you have merely to cut through the skin and fat, and tear a little cellular tissue with your nail, and you come down upon the sac.

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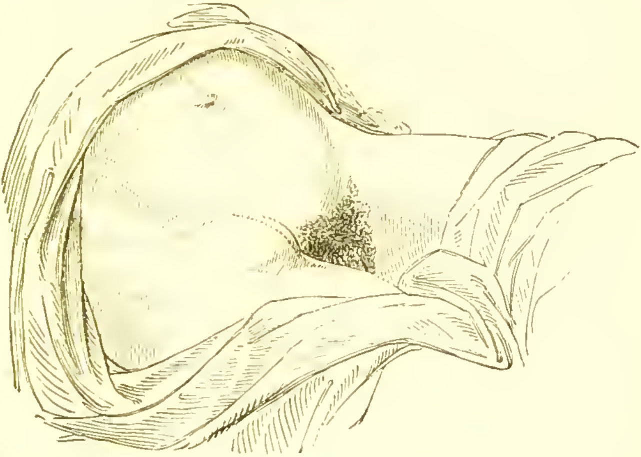
* Figs. 15 and 16 are taken from Fergusson's *Practical Surgery*, and Fig. 17 from Liston's *Operative Surgery*, by permission of Messrs. Churchill.

FIG. 16.



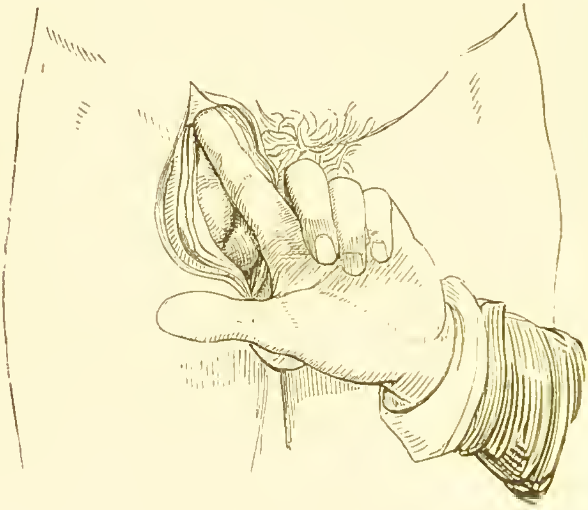
But how will you recognise the sac? The sac is a bluish-looking membrane which has no arborescent vessels upon it; for although these are always described, they do not often exist. You will find if you look at the sac that it is transparent, and you can generally see through it the bowel, and often some fluid, and it is not glistening. Remember once for all that the outside of the peritoneum, although it may look a little shining in the wet which is sponged off it, is not glistening in the same sense as the inner surface of the peritoneum. Having cut down upon this blue membrane, which is the great guide, you have only to tear the tissues a little round it, and isolate the sac sufficiently to make sure that you have reached it.

Now comes the question, which used to have more importance than it has now, Will you or will you not open the sac? I am not going to detain you with the controversy which raged a good many years ago as to which was the best practice. For myself, I think it is much better in the majority of cases to open the sac. If you are taking, as you ought to take, every reasonable antiseptic precaution, having your fingers and instruments well dipped in carbolic, and sponging the part with carbolic lotion as you go on, it is much better to open the sac and ascertain thoroughly what the condition of the bowel is. Still, if it is a comparatively recent case you may feel justified in operating outside the sac. In that case you would divide the stricture with a hernia-knife, wherever you find it. In the inguinal region you cut upwards, to be parallel to the epigastric artery; in the femoral region you cut inwards, in order to divide Gimbernat's ligament.

Supposing that after dividing the stricture as far as you can outside, you still cannot return the bowel, the fact being that the constriction is more or less in the neck of the sac. Then of course it is a case for opening the sac, and that is the critical part of the operation. In the great majority of cases, fortunately for the surgeon, there is a little fluid in the sac; therefore, when he picks up the sac and nicks it against the forceps with his scalpel, the moment he has divided the sac out comes a little fluid, which is a great protection to the bowel within. But every now and then, in quite exceptional cases, there is no fluid, and the sac may possibly be adherent to the bowel within. Under those circumstances it is exceedingly difficult to distinguish and separate the sac from the bowel, and I can conceive that it might be possible to open the bowel inadvertently. Still, with care, I think that should not occur, and practically it does not occur. Then, having made a little nick into the sac, you must open it on a director sufficiently to pass your finger in.

It is a very important point not to open the sac too largely; if you do, and if it is a large scrotal hernia, out will come two or three coils of intestine, and there will be a great difficulty in putting them back, and the exposure of the coils, as shown in Fig. 17, will be bad for the patient. You ought not to make a larger opening than you can conveniently plug with your finger. Having got your finger inside, you pass it up to the stricture, and you can either use the nail of your finger, if you have a tolerably long one, or, if you prefer it, you may use a flat director, and slip it along your finger until you guide it beneath the stricture. If it is an inguinal hernia, you will slip your hernia-knife along the groove, and just divide it upwards; or if it is a femoral hernia, you will turn the knife sideways. A very little notch is quite sufficient, anything like an incision being quite unnecessary. The

FIG. 17.



result will be that you will be able to return the bowel.

Now comes the question, Shall you in all cases return the bowel as soon as you divide the stricture? In the great majority of cases no doubt you will. In the case before us Mr. Pollard did so, and then he proceeded to perform a radical cure. But there are circumstances which would prevent your returning the bowel. You find perhaps, when you have opened the sac and exposed the bowel, that it is simply congested. There can be no question about returning that; it is a piece of healthy bowel perfectly glistening and smooth, but congested, and no one can hesitate about returning it. But supposing you have something more than that—that it is not merely congested, but that it is black, and that the surface is not quite smooth—shall you return it or not? The great majority of surgeons would say that you should put it back, though I believe you may occasionally leave the bowel down to take its chance with advantage. But supposing it to be still worse than that—that it is actually gangrenous, with grey spots upon it, and that there is, what I should lay a great deal of stress upon, a smell of fæcal matter. Then there can be no question about it; you have there a sphacelated bowel which is practically dead, and will give way if you return it. Therefore you ought to lay it open and establish an artificial anus, about which I will say a word presently.

What is the effect if you put the bowel back in a damaged condition? does it do any harm? Well, that is the mischief. There is no peritonitis in the abdomen—Mr. Hutchinson settled that question a good many years ago—so long as the strangulated hernia is outside it. If you have unhappily the opportunity of seeing a patient die with an unrelieved strangulated hernia, you will find in all probability that there is no general peritonitis; there may be acute peritonitis and inflammation, no doubt, in the sac of the hernia, but in the cavity of the abdomen there is none. But if you put back a piece of damaged intestine, and the patient dies in three or four days, as patients are apt to do, with peritonitis, you can have no doubt that putting back the piece of bowel has set up the peritonitis, and therefore I prefer to leave damaged bowel down in the hope that if it recovers it will go up spontaneously, and with the certainty that if it gives way it will do so outside the peritoneal cavity.

Let us take a case where the bowel actually gives way after it has been returned. Is that necessarily fatal? It certainly is if the fæces get extravasated into the cavity of the abdomen; there can then be but one end of the patient; he will have acute peritonitis, lasting forty-eight hours or so, and then die. But, fortunately, every now and then we find that this occurs: you put back a piece of bowel which you are a little doubtful about; you take care to put it gently back and leave it as close to the ring as you can; it forms an adhesion to the margin of the ring, and then it gives way, and the result is that you have fæces coming out of the wound, and two or three days after the operation you have the formation of what is called a fæcal fistula. Now, although that is very alarming to the friends and those who do not understand it, as a matter of fact a fæcal fistula occurring in that way is not very serious, because it generally gets well of itself; the fæces are poured out of the wound for a few days and then gradually diminish and dry up, the wound closes, and the patient is as well as ever.

Many years ago I had the opportunity of seeing a specimen—I think it was brought from the Middlesex Hospital to the Pathological Society—from a patient who had had some years before a fæcal fistula, and then happening to die in the hospital from something unconnected with the hernia, the opportunity was taken to examine the parts. They showed that there were very firm adhesions of the bowel to the margin of the ring, which in the course of years (I think five or six years had elapsed) had become stretched; and so the piece of bowel, although no doubt it was at one time bound down firmly to the femoral ring, had afterwards become loose, and was as freely movable in the abdomen as you would expect a piece of bowel to be. Clearly understand, then, that a fæcal fistula is not a very serious thing. It is nature's way of relieving a piece of damaged bowel. But unfortunately you cannot always count upon the bowel becoming adherent to the margin of the ring before it gives way, and under those circumstances the fæces become extravasated, and generally a fatal result ensues.

Then, coming to a further point, suppose that we have a strangulated bowel so completely gone that we see that it is not justifiable to return it. It is offensive to the smell, quite soft, and there may be grey spots about it, and you make an artificial anus. There seems to be a great diversity of opinion as to whether in making an artificial anus you should divide the stricture or not. It seems to me that you must divide the stricture: because, if you do not, I cannot see how the fæces are to pass. You must divide it with as little disturbance as you can of the parts, then lay open the bowel, and leave it alone. Stitching, I believe, is quite unnecessary in an ordinary case. Of course, if there is a large coil of intestine, it may require to be cut away; but these cases generally occur in femoral herniæ, where there is a small knuckle of intestine, and all you have to do is to lay it open, and to allow the fæces to pass.

Now, in these days of advanced abdominal surgery, you may think that it would be possible, after excision of the affected portion, to stitch the bowel together at once, and restore continuity. I should not advise you to try it. It is not like a case of wound, where you have a healthy bowel to deal with. You have a more or less inflamed and softened bowel, and if you were to attempt to stitch together the two ends at once, you would probably have the stitches breaking out. Then, again, there is this great difficulty, that the opening through which you would have to draw the bowel is so small that it would be very difficult to manipulate it. And again, the most important point of all is that the condition of the patient is one which forbids any prolonged operation.

If the stricture is relieved, as it ought to be, the fæces will pass out of the upper end, and nothing will find its way into the lower end, and that is a condition from which the patient may well recover. You know that nowadays we do not think so much of having an artificial anus in front; it is the fashionable way of doing inguinal colotomy. But there is this great difference between an accidental artificial anus and colotomy, that it is small intestine with which we are concerned, and you can never tell at what part of the small intestine the accident may occur; consequently you may find that semi-digested food will come pouring out of the groin, and the patient will not be nourished. Under those circumstances, of course, it will be a question, supposing that the patient should get over the primary danger of the hernia, of restoring continuity, and it is obvious that what you want to do is to get rid of the *éperon*, or spur, which prevents the fæces from passing along the lower part of the bowel. We do that by putting on a pair of forceps, and nipping the piece of bowel. There is a screw arrangement in what is called Dupuytren's enterotome; but it is nothing more than a large forceps, which you screw upon the spur until it sloughs away, and when it has sloughed away there is of course not much difficulty in doing a plastic operation outside, and so getting a restoration of continuity.

ON

MALIGNANT DISEASES OF THE BREAST.

I HAVE to-day to speak of malignant diseases of the breast, and of course in malignant disease we nowadays include the sarcomata as well as the carcinomata. Until within the last ten or twelve years it was hardly recognised that the sarcomata formed a group by themselves, almost as malignant in many features as the carcinomata, so that you will find in all but the later books and lectures on diseases of the breast that they are not mentioned as a specific class, but clearly they must be included among the malignant diseases. We do not see them often. I show you a photograph of a case of sarcoma of the breast which we had here a short time ago. Some of you may remember a woman with an enormous growth, a very large mass of disease of a soft character, in whom there was no enlargement of the glands of the axilla. That is a point to which I wish to call your attention as distinguishing the sarcomata from the carcinomata—I mean as regards the breast—for I do not think that the rule about sarcoma not involving the lymphatics holds good in every case.

You probably know that one form of sarcoma particularly, the melanotic, involves the lymphatics rather than the vessels; but as a class the sarcomata do not readily involve the lymphatic glands; certainly in cases of sarcoma of the breast you may be sure that you will not have the lymphatic glands enlarged. Given, then, a rapidly growing soft tumour of the breast, the chances are that if the glands of the axilla are not enlarged it is a sarcoma. It makes no difference with regard to the treatment, but it is important, of course, with regard to the prognosis. You must remove the growth, but the great misfortune is that the tendency to recur locally is very great. In the photograph that I show you you see the fourth growth which the poor woman had had removed. She had certainly been operated upon twice, and, I think, three times by another surgeon, and then he declined to do a further operation. The woman came here in a bad condition, with a very large growth, which had already ulcerated through the skin, and I thought it quite justifiable to operate once more. I think it is a pity to leave an unfortunate woman with a great mass of disease protruding immediately below the face to end her days in misery. Of course, it may be that you will kill the patient by the operation; and I really can hardly say that it is to be regretted. But as a rule they do not die; they do remarkably well. If you clear away the whole of the soft growth, the wound granulates up tolerably easily, and for some weeks, or perhaps months, the patients are relieved from their trouble. This was an example of spindle-cell sarcoma which had ulcerated through the skin, and which must have led to great misery before it terminated the patient's life. I do not know what the end of the case was, but certainly the patient went out of the hospital relieved.

That is a comparatively rare form of malignant disease. The common form, which we see so much of in the hospital, is of course the ordinary scirrhus. The cases which come to the hospital as a rule are well-marked cases of scirrhus, and you are sometimes rather surprised that they have not been recognised before, and that the patients have not been treated earlier. But that is too often the case with hospital patients; they go on much longer than they ought to do before they apply for relief; and I want to impress upon you, as practitioners who will see these cases in quite the early stage, to observe the importance of recognising and treating scirrhus early.

Now let us go into the diagnosis of a case of scirrhus before referring to the treatment. A woman varying in age from thirty to fifty (about forty is the most likely age) has a little lump in the breast. It is astonishing how long women will go on with a lump in the breast and not notice it. In performing their ablutions they must necessarily manipulate the breast to a certain extent; still they do not notice that there is a lump in the breast until possibly there is a slight accidental knock, or something of that kind, which calls attention to it, and then they always say, I believe with a perfectly good conscience, that the lump has followed the blow.

That is so commonly said that you cannot but suppose that sometimes it must be true. I believe that a patient with a cancerous diathesis will start a cancer upon receiving a blow; but, on the other hand, I am equally sure that there are cancerous tumours of the breast which have been growing for weeks or months without being found out. Sometimes there is a shooting pain in it that calls attention to the part, and then it is found that there is a little lump. If the lump is on the surface of the breast, as it most generally is, immediately beneath the otherwise healthy skin, in a very few weeks it will produce an effect, which we all recognise as so exceedingly characteristic—namely, the dimpling of the skin when you pinch it up with your finger and thumb. But then you must bear in mind that there are cases where scirrhus is not on the surface of the breast, but in a deep part. I was particularly struck with this fact lately when holding an examination for the M.B. degree of the University of London. We had a woman there for the examination who had undoubted scirrhus of the deep portion of the breast. I say undoubted, because she had a number of enlarged glands in the axilla, and I was rather surprised that gentlemen, presumably of advanced knowledge, failed entirely to recognise what the case was. They could not believe it was scirrhus because there was no dimpling of the skin, and of course no retraction of the nipple.

As to the retraction of the nipple ; if the growth is on the surface of the breast, and just beneath the nipple, in a few weeks you will find that the nipple is somewhat fixed, and in a few more weeks it is drawn in. But if the disease is not in the central portion of the breast at all, but on the inner or the outer side, then it follows as a matter of course that you have no retraction of the nipple whatever. You have first, then, the hardness, then the tendency to involve the skin, and then the retraction of the nipple. Then, I think, I should put the pain next. Now, pain in a scirrhous breast is very marked, being of a stabbing or lancinating character. You need not ask whether it is a stabbing pain or not ; the patients will tell you without prompting that it is just as if a knife were being run into them, and you cannot have a better definition of a lancinating pain.

Given, then, these features, I think you ought not to hesitate about pronouncing the case to be scirrhus. Of course, the lymphatic glands are not always involved, and to wait for the glands of the axilla to become involved is to do your patient a direct injury; and it is to be regretted that medical men are apt to wait for the involvement of the glands before making their diagnosis. Of course the patient is then so much the worse, because not merely have the glands become infiltrated with cancer, but no one can tell how much further beyond the glands the lymphatics may already have become affected. If you have any doubt about a case, make an incision into it, which will at once settle whether it is cancer or not. Do not wait and dally with a case, and apply belladonna and things of that kind, which are all very well for simple tumours, but cannot in any way affect the growth of cancer, which all the time is making progress and doing the worst thing possible for the patient.

We will presume, then, that you have a case which does not involve the glands, and that you are going to operate. Shall you remove the tumour, or shall you remove the breast? and if you remove the breast, shall you remove also the axillary glands? Now I think that where you have a single outlying nodule of disease at the margin of the breast, it is justifiable to cut out that nodule and leave the breast; but in by far the great majority of cases where the mammary gland is involved more or less in the central portion you ought to take out the whole gland.

You see here the breast I removed last Wednesday, and you will observe a nodule of scirrhus close by the side of the nipple. You will remember that this was a case of a somewhat unusual character, the nodule being so entirely localised and the skin so firmly bound down to it. You can see here the hard pear-like appearance of scirrhus, and you will see it better if I make a fresh section. In a recent specimen you may notice that the surface becomes somewhat concave from the contraction of the tissues. In such a case I advise you to remove the entire breast, and let me say it is not such a simple thing to remove the entire breast as some men suppose. I am sure that in many operations the margin of the breast is left, and it is a great mistake to leave it. The central mass of the breast with disease in it is easily recognised; but when you get to the margin it is not quite so easy to see it, and the operator, if working boldly with a knife, may readily cut through the margin of the breast, and leave it behind. That is why you will notice in my operations that I always use the fingers a good deal after I have first reflected the skin with the knife. I usually reflect the skin by a double incision, and then with the fingers tear away the skin until I feel that I have got to the margin of the breast. You can with a little practice with your finger recognise the edge of the breast, which is perfectly regular, and pull it up with a nail, and dissect or tear it away from the pectoral muscle. There is also another advantage, that we get less hæmorrhage. You may notice that I have seldom to tie or twist more than one or two vessels after removal of the breast.

Now comes the question, Shall you or shall you not turn all the glands out of the axilla? On this point I may say that there has been a good deal of discussion of late years. Twenty years ago or more, if the glands in the axilla were enlarged, that was considered to negative an operation. You will find that surgeons like Mr. Syme and Sir William Fergusson, if they had a case of cancer of the breast with enlarged glands, declined to operate. We have improved upon that, because whenever the glands are enlarged we do not hesitate to take them away. For the last ten or fifteen years all operating surgeons have come to the conclusion that it is perfectly easy to turn out these glands from the axilla, and clear out as much as may be necessary—in fact, to clear the axilla.

But the question that has been discussed a good deal by Mr. Mitchell Banks of Liverpool, and by other surgeons, is whether in all cases it would not be wise to turn out the whole of the glands of the axilla, because it is possible that they may be involved. I am perfectly certain that you may have glands involved and not be aware of it; I mean that it is impossible through the skin to be always sure whether there is enlargement of the glands or not. Therefore I always make a point of putting my finger into the axilla and under the pectoral muscle, and feeling whether there are or are not any perceptible glands. If there are, I take them away. At the same time I do not think it is justifiable in a simple case of scirrhus—a small nodule of scirrhus of the breast with no perceptible enlargement of the glands of the axilla—to turn out the whole contents of the axilla. Mr. Banks thinks that it is. He says it is better to kill a few patients by an operation of that magnitude than to leave a large number of them with possible germs of cancer still in the axilla. But I think you can make out pretty clearly whether there are germs or not, and if there are you ought certainly to remove the glands.

With regard to removing the glands, you have seen me do it many times, because the cases which come to the hospital are generally, I regret to say, cases in which the glands are already involved, and you will have noticed that after making an incision upon the edge of the pectoral muscle, I generally turn out the whole mass of the glands with the finger. I think in that way you run much less risk of doing harm than you would by using the knife. If you use the knife freely, you are apt to cut more vessels and to get considerable hæmorrhage. At the same time, you have to be cautious, because the first thing you come upon is the axillary vein, and if you tear that it is a rather serious matter. I have torn it before now, and had to put two ligatures upon it, which is a thing one would rather not do. Then, of course, immediately behind the vein there is the axillary artery. I do not think you could tear the artery in that position, but it is possible to tear a branch of it, particularly the subscapular artery. You should be careful about the position of this, and avoid tearing it or the corresponding vein, which, as I have said, would give rise to a good deal of hæmorrhage. With these precautions, I think you may turn out the contents of the axilla, as far as they are diseased, with very great advantage to the patient. You may have seen me even put my finger up under the pectoral muscle, and get the glands out from the posterior triangle. There was a case last summer in which I did that.

You will bear in mind that those glands are sometimes enlarged as a consequence of axillary disease. Some writers advise that the pectoral muscle should be divided ; but I have never found any necessity to do that. I may say, however, that I have removed the greater part of the pectoral muscle before now, where it has been involved in the cancer. Because you must remember that a cancerous breast tends to form adhesions to the pectoral muscle ; and not unfrequently in lifting up the breast you find that some fibres of the superficial portion of the pectoral muscle are adherent to it, and then you tear or divide them, and pull them away. But I have known cases, particularly cases of soft cancer of the breast, where the whole of the pectoral muscle was involved ; and then it is necessary, if you are to do anything at all, to cut the muscle away, which makes it a serious operation, attended with very considerable risk, and it should only be undertaken in extreme circumstances, where there is a large mass of disease which is fungating or tending to fungate, and which you are obliged to remove as a temporary relief for the patient. Those cases are relieved for the time, but I have never known one that lived many months after such a serious operation.

So much, then, for the proceeding with regard to the removal of the breast. I ought perhaps, in speaking of the operation, to have made some remark with regard to the incision. The ordinary incision which I suppose most surgeons make is the double elliptical incision, following pretty closely the fibres of the pectoralis. Mr. Beck, however, has used another incision, which you will find mentioned in Mr. Erichsen's book. Here the elliptical incisions surrounding the nipple are carried across the pectoral to meet another going straight up into the axilla. There is no doubt that this incision gives better drainage, and it is because I have been dissatisfied with the drainage afforded by the elliptical incisions, that you may have noticed latterly in my cases I have put the drainage-tube not into the wound at all, but through the posterior flap. I think either way—putting the tube through the posterior flap or using Mr. Beck's incision—is better than the old method. If you have the elliptical incisions, and the drainage-tube is put in the usual way at the top, it is difficult to make the patient lie down sufficiently to cause the fluid to gravitate from that part. Theoretically it is all very well; the patient is supposed to be perfectly flat in bed, and then no doubt the drainage-tube would work; but practically we know that the patient is supported with pillows, and it is difficult to get thorough drainage. If you put the tube through the posterior flap you can have efficient drainage, or if you put it, as Mr. Beck does, at the end of the incision, you have a large amount draining both from the axilla and from that portion of the wound.

Of course there may be other incisions. It is absurd to suppose that you can remove every breast by the same incision. If the skin is involved at any particular part you must meet that by surrounding it with an incision. Many times before now I have made a vertical incision, surrounding the nipple and including the skin which happened to be attached above or below the nipple in the central portion of the breast. A surgeon must be prepared to modify his incisions according to circumstances, so as to make quite sure that he gets rid of the whole of the disease. The great point is that the whole of the breast should be removed, and all the glands, so far as they can possibly be reached; because I may remind you that there are glands which you cannot reach. If you have cases of cancer of the inner side of the breast, the chances are that the glands in the mediastinum are already diseased. You cannot help that; it relieves the patient, and it certainly does good, to remove the breast, but unfortunately in these cases too often recurrence takes place in the lymphatics of the mediastinum, and the patient dies from that cause.

With regard to the healing of the wound, we always aim, of course, at immediate union. Everybody is pleased when the parts unite by first intention, and the patient gets well and is out of the hospital in perhaps a fortnight. I am not sure, however, that this is always the best thing ultimately for the patient, and for this reason—one too often finds in those cases which do so well little nodules of disease springing up soon afterwards.

I have been much disappointed in some of my cases which have done admirably, the patients leaving in a fortnight quite well, to find within three months little nodules of cancer springing up here and there, not necessarily in the cicatrix, and therefore not showing that anything had been left behind, but springing up in different positions.

I am not at all sure, though I do not lay it down, but merely express an idea passing in my mind, whether many of these cases would not do better if we let the wound suppurate, and therefore possibly allowed all these little bits of diseased tissue to be thrown off in the discharges. I believe that some of these cases of suppurating wounds after removal do better in the end than the very pretty cases of immediate primary union.

Then supposing that the patient recovers from the operation, for the risk is not great, though of course if you have to remove the axillary glands it is increased, what are the prospects in regard to recurrence? Mr. Morrant Baker some years ago collected a number of statistics from Sir James Paget's practice, and those statistics give fourteen months as the average date of return in scirrhus, and seven months in medullary disease. It is notorious that medullary cancer of the breast is more rapid than scirrhus, both as regards primary growth and as regards recurrence. This is a point of very great importance, because naturally the patient and her friends before the operation often say to you, "What are the chances for this patient? If we go to the expense, trouble, and anxiety of an operation, what can you promise?" Well, you cannot *promise* anything. Anybody who holds out a prospect to the patient of absolute cure in a case of cancer is behaving very unfairly to her. All that you can say is that you may hope for about two years, on the average, of complete relief. You cannot promise or hope for more. Mr. Baker's statistics give fourteen months, but I think we may take it a little longer for the average run of cases if they are taken pretty early.

Then, unfortunately, there may be nodules forming in the cicatrix or in the neighbourhood; there may be a re-development which may admit of further operative interference; there may be enlargement of the glands, if the glands have not been already removed; still it does not follow that the patient cannot be relieved even then. I have operated twice or three times in cases of scirrhus. Only the other day I operated upon a lady for the third time. There was a large nodule which had sprung up in the cicatrix beginning to ulcerate, and I was afraid that if I did not remove it it would give considerable trouble. Every now and then, indeed, you are fortunate enough to find a case which goes on very much longer; the patients live for many years afterwards, and you really seem to have eradicated the disease, but you cannot tell beforehand which those cases will be.

Then let us take the prospects for a patient as regards the whole history of the case. In the first place let me quote Sir Astley Cooper's views with regard to that point. He gave, as the result of his experience, two years for the full development of cancer of the breast, and from six months to two years more as the limit of a patient's life. I think he is quite right with regard to the two years for the development of cancer in the breast, but I think that the period of from six months to two years is rather low with regard to the limit of the patient's life, because, although you may have recurrence in a cicatrix or elsewhere, those patients do not necessarily die; they live on for some months, although there may be a certain amount of suffering from the disease.

Mr. Sibley many years ago investigated the statistics of the cancer wards of the Middlesex Hospital, and let me say that the cancer wards of the Middlesex Hospital differ from ordinary wards in the fact that the patients admitted with cancer are kept there till they die. Mr. Sibley's statistics gave fifty-three months as the average of life when the primary disease is removed, and only thirty-two months when the case is left to nature. That is a very important point. You are asked by the patient, and particularly by the friends, "Will the operation prolong the woman's life?" I think it will. You may say that, provided the first risk of the operation is got over, the patient's life will certainly be prolonged, and the statistics I have mentioned go to show that this is the case. But those statistics are statistics of all cases of cancer taken together. Mr. Baker's statistics, to which I before alluded, separate scirrhus from medullary cancer, which of course is rather important, because they are different groups of cases, and they show that when a case of scirrhus is left alone the average duration of life is forty-three months, and when it is operated upon fifty-five months; and that in the case of medullary disease when left alone the average life is twenty months, and when operated upon forty-four months, so that the gain is very considerable. If surgeons, too, would only be impressed very strongly with what I said at the beginning of the lecture, that cases should be taken early and operated upon before the glands are affected, I believe we should have even better results than are here recorded.

People, too, have not the fear of operations that they used to have, because nowadays, what with anæsthetics and antiseptics, patients have so very little suffering that they hardly seem to appreciate surgery as they ought to do; and when it comes to a question of signing a piece of grey paper they sometimes seem to think that they have not had enough for their money.

Medullary disease, of which we have not had a case lately, is of course the most rapid form of disease in the breast, and it is also the most serious for the patient. It is one in which you find the skin becoming rapidly implicated and the glands in the axilla extensively involved. That is what distinguishes it mainly from cases of sarcoma. Given then a case of medullary disease where the skin becomes rapidly brawny. and where, if you do not interfere, it gives way, you will have a large fungating mass, which constitutes the *fungus hæmatodes* of the older authors. It is a thing that you hardly ever see now, because both surgeons and patients do not allow cases to go to that extent, but that is what would happen if the case were not soon relieved. It is because of this tendency to fungate that I strongly advise an operation in these cases, although you know that you cannot save the patient's life, but you can get rid of the mass of fungating, offensive, discharging disease, which will make the patient's remaining weeks of life very miserable.

In a case of that kind you should operate freely, and clear away the whole of the skin that is involved. There, again, modern surgery allows us to take liberties, because by the lead plates of Sir Joseph Lister you can draw the skin together and transplant a flap when necessary, or if there is an open wound its granulation can be aided by grafting portions of healthy cuticle. In these cases always take away the glands, because they are infiltrated. And there is this peculiarity with regard to certain of these medullary cases, that it is not merely isolated glands, as in scirrhus, that are affected. In scirrhus, when you enucleate the glands you can see each gland separately; but in the case of medullary cancer the whole mass of tissue is infiltrated by disease, and you have really to clear out glands, cellular tissue and all, and expose the axillary vessels very freely, and that of course is one of the dangers of the operation.

ON

WHITLOW

We do not, as a rule, admit cases of whitlow into the wards, but having recently had two cases under my care, I take the opportunity of making a few remarks upon the subject. I need hardly say that it is an important one, as cases of whitlow are common and require judicious treatment; but I must remark that I have found with surprise, on looking up a few of the surgical authorities, the very inadequate and, in some cases, erroneous way in which the subject is treated.

Taking the simplest form of whitlow, or *paronychia*, first; it is a localised inflammation of the skin of the finger, of an erythematous or erysipelatous character, which is generally due to an animal poison. Yet it is not seen as the result of an ordinary post-mortem examination of a human body, but is met with on the hands of cooks and poultry-dealers who have to do with game which is "high." Some slight prick or scratch is sustained, and, a few hours after, the finger begins to tingle and the skin to redden without any great swelling, and this redness or erythema spreads up the finger, and, if not checked, up the hand. This is very different from the lymphatic inflammation we are all too familiar with as following "post-mortem wounds," and much less serious; for, though it may spread if not treated, its tendency is to disappear without any serious harm happening. The treatment is, however, simple enough, for nitrate of silver checks it at once if applied as a twenty-grain solution in nitrous ether, or lightly pencilled all over the reddened part and a little beyond with the wetted stick of lunar caustic.

The next in severity is the superficial or subcutaneous whitlow, found usually about the nail or the tip of the finger, but occasionally in other parts—due to some slight injury or irritation, such as is likely to occur in the hands of nurses and housemaids, who constantly use blacklead and soft soap. Here matter rapidly forms, with the usual painful throbbing, but is ordinarily quite superficial, and can be readily evacuated without the least pain by incising the cuticle, which is perfectly insensitive. Occasionally, however, when a foreign body has found its way beneath the nail, the matter forms there, and gives rise to excruciating agony from the tension beneath unyielding structures. Judicious cutting away of the nail will relieve this if near the margin, but if near the base it is much better to pare down the nail with a sharp knife till the matter is let out, than to resort to the unnecessary cruelty of removing the entire nail.

The third kind of whitlow is really an acute necrosis of the terminal phalanx, following periostitis and suppuration beneath the periosteum, just as it does in the case of a long bone. A very slight injury may set it up—the mere prick of a needle or pin, which is forgotten before the mischief shows itself. After some hours' uneasiness, with slight swelling of the terminal phalanx, the pain becomes acute and throbbing, and entirely prevents the patient's sleeping. If timely relief be not given, matter will very slowly make its way to the surface of the finger, but never up the sheath of the tendons, and, when discharged, will leave the greater part of the phalanx bare and dead behind it. A timely and free incision is the only mode of saving the phalanx, and cannot be resorted to too early, for, if no matter be present, the inflamed periosteum will still be divided with great relief to suffering. The finger should be held firmly on a table, and the surgeon, entering his knife just above the transverse interphalangeal mark in the skin, should cut boldly down to the bone in its whole length from base to apex.

When, as so often happens, these cases have been treated domestically with "soap-and-sugar" and poulticing until the end of the finger is riddled with sinuses, there is nothing to be done except to extract the necrosed phalanx as soon as it is loose, and to bring the finger into shape by careful water-dressing applied in strips, as in the woman lately in my wards. In the great majority of cases, the base of the phalanx (which, I may remind you, is its epiphysis) survives the mischief; and this is important, because it gives attachment both to the tendon of the deep flexor and also to the sheath of the tendons; but, even when it necroses, the sheath escapes, and matter does not find its way along it. The phalanx being gone, the soft tissues contract, and the nail becomes considerably incurved, leaving a somewhat shortened but very useful finger.

Then we have inflammation of the skin and subcutaneous tissues in any part of the finger, and this may lead (as in the boy in No. 1 Ward last week) to the true thecal abscess, or to necrosis of any one of the phalanges. Originating often in some slight puncture or poisoned wound, the inflammation is very acute, and the tension and pain severe. Leeching may be of service in the early stage of the disorder, but, if it is not shortly arrested, incisions will be necessary; and it is here that mistakes are often made. The incisions, whether made along the centre or the sides of the finger, ought *not* to open the theca or sheath of the tendons, for if the sheath is opened in this position the tendons invariably slough, and the patient is left with a stiff finger. For this reason, incisions on each side of the finger are safer than one in the centre, that may unawares let out the tendons, which will look perfectly healthy at the moment, but soon become sodden and softened.

In the patient I showed you last week I found that, notwithstanding an incision which had temporarily relieved him, matter had formed in the sheath and had found its way into the palm. This was shown by the tension both of the palmar and of the dorsal surfaces of the hand—the latter being a very important guide in the diagnosis; and I proceeded to evacuate the matter by making an incision through the palmar fascia, over the head of the metacarpal bone. You will remember that the superficial palmar arch never comes farther forwards than the middle one of the three creases or "lines" found in the skin of the palm; and that the digital arteries arising from it, in the spaces between the flexor tendons, bifurcate some half-inch from the webs of the fingers. There is plenty of room, then, for a sufficiently free incision in the centre of any of the fingers below the transverse crease which marks off the finger from the palm, or, as I have said, over the head of the metacarpal bone. Matter flowed as soon as I had introduced my knife through the palmar fascia, and I enlarged my incision towards the wrist, upon a director, to the extent of nearly an inch without seeing anything of the flexor tendons. The result has been that the boy has gone out in a week with a perfectly useful finger, whereas, if I had opened the theca and exposed the tendons, they would now be separating, and the finger would certainly become useless.

The synovial sheaths of the flexor tendons of the thumb and little finger are often, though not always, in direct communication with the synovial membrane of the annular ligament of the wrist, and hence matter is rapidly conducted in this way up to and, if not relieved, into the forearm.

As regards removal of a finger, a great deal will depend upon which finger it is. The forefinger on the right hand is so important for holding the pen, that even a stiff forefinger is better than none. The middle and ring fingers are, however, comparatively unimportant, and if stiff are apt to get in the way and become an encumbrance. The little finger is seldom affected by whitlow, and will not materially affect the usefulness of the hand if stiff. As regards the thumb, too much care and trouble cannot be expended upon its preservation. If the terminal phalanx is necrosed, it can be extracted as in the other digits; and if the first phalanx, or even the metacarpal bone, is necrosed, either may be extracted, and leave a shortened but very useful thumb.

ON

SORE THROAT.

In severe weather, such as we have been having lately, sore throats must be extremely common. I therefore take the opportunity of saying a few words to you upon the subject.

The commonest form of sore throat is acute catarrhal pharyngitis—that is to say, a person who has, perhaps, a delicate throat, being exposed to cold, finds that he has some little difficulty in swallowing and a little pain in the throat. On looking into such a throat as that, you will see that there is a general injection of the mucous membrane, and not much else. It is a simple matter, which will get well under ordinary treatment; by which I mean gargling with hot milk and water, applying a poultice round the neck, and perhaps taking a purge, if it is required. The only thing that I would say with regard to this simple matter is that you should be careful with regard to the question of the possibility of its being an infectious sore throat. I mean it may possibly be the beginning of scarlet fever or of diphtheria. I need hardly point out how essential it is that you, as practitioners in families, should take every precaution to avoid any possibility of error, and to isolate immediately any child whom you suspect even to be sickening of either of those diseases.

Then next in frequency to this simple pharyngitis I would put acute tonsillitis, or quinsy. You find that the patient, more often a female than a male, and a young person, usually, of delicate habit, has been exposed to cold and finds difficulty in swallowing and in speaking. The thickness of the voice is very characteristic, and when you have once heard it you will recognise it directly. That patient, you will say to yourself, has got inflammation about the tonsil, and now, on looking into the throat, you see at once that the tonsil is considerably enlarged, the fauces are injected, but the thing that immediately strikes you is that the palate is pushed forward by the large tonsil behind it. Here let me remind you a little of the anatomy of the part—viz., that the tonsil in health is situated between the anterior and posterior pillars of the fauces, and that ordinarily we see only a small portion of the tonsil projecting towards the middle line. Now, when the tonsil becomes inflamed, the whole of the organ becomes enlarged and forms a projection behind the soft palate, pushing forward the latter, which is somewhat red and discoloured. You find in these cases that the patients have great difficulty in swallowing, and a good deal of pain, often extending up the Eustachian tubes to the ears. and the consequence is that they endeavour to avoid swallowing as much as possible, even the saliva, which they hawk up with considerable difficulty, and that they find great difficulty in taking food. It is very important to bear this in mind, because these patients require food and nourishment, and unless you are careful to support them by properly prepared liquid food, you may find them running down and getting into a very dangerous condition. Now, in the early stage of acute tonsillitis—if seen in time—I have great faith in the administration of an emetic—I mean some warm mustard and water. It is very remarkable what an effect that has in producing revulsion and stopping the inflammatory action about the tonsil; but, unfortunately, one does not often see the patient early enough. The inflammation has got beyond the stage at which the emetic could stop it, and you have therefore to treat the disease as you find it.

In acute tonsillitis there can be no question about the propriety of warm treatment—hot applications inside and hot poultices outside. With regard to medicine, I believe the best you can give the patient to be belladonna in repeated doses, and I should give drop doses of the tincture of this drug every hour, and should also apply belladonna externally beneath the poultice, which gives great relief to the painful swelling. If, however, the inflammation does not subside within twenty-four hours, you may be quite sure that an abscess will form.

I want to say a word as to the place at which that abscess should be opened. No doubt patients have lost their lives before now from an abscess in quinsy suffocating them; and, again, there are plenty of cases on record where, at the last gasp, as it were, the abscess has burst, and the patient has been immediately relieved and got well. But the surgeon can do a great deal to relieve by a judicious puncture at the proper place and in the proper manner—that is, by making his puncture with a guarded bistoury through the soft palate into the tonsil. Do not try to get round the anterior pillar of the fauces, because, if you do, you will have your knife in an oblique direction, somewhat dangerous, perhaps, for the carotid artery; but take my advice, and go straight back through the soft palate, and I will undertake to say that no effort on your part can possibly bring the knife into any relation at all with the carotid vessels. Of course, it is difficult to be quite sure in all cases that matter has formed, and it may be that on making your puncture into the tonsil through the soft palate nothing but blood escapes. So much the better for the patient. The puncture will relieve the congestion; it may possibly save the formation of an abscess; or if it should happen that an abscess forms later, the matter will be able to find its way out perfectly easily through the puncture you have already made. But in a good many cases you will find the matter already there, if you will make the puncture, as I say, through the soft palate, and by cutting a little upward open the upper part of the tonsil. As soon as the abscess is opened the patient is relieved. He can swallow, he can speak, he begins to take food well, and very soon will convalesce.

Now, the third kind of acute inflammation of the throat to which I will call your attention is the most dangerous of the three. It is acute œdema of the pharynx, which tends to spread down to the larynx, making it so dangerous to the life of the patient. You will find that a patient is ill, is hoarse, and when you look into the throat you see not very many signs of inflammation, but a general œdematous condition of the parts: the uvula is twice its proper size; the pillars of the fauces are considerably swollen; and if you put your finger in, you will probably feel that the tissues about the epiglottis also are considerably swollen. The danger of these cases is that the œdema may so involve the upper part of and extend down into the larynx, that the patient's breathing may become very much embarrassed, and, if not promptly relieved, may cause him to lose his life. When you are called in to a case of that kind there is very little difficulty in recognising it, if the disease has already involved the air-passages, because the breathing of the patient is so very characteristic. The stridor, the noise which the patient makes in the drawing in and out of his breath, is so characteristic that you can very often recognise it, in fact, before you get into the room. Now, a case of that kind is of the most urgent description. You must remain with that patient until you have relieved him, and the best way to do this will be to make some punctures with a sharp bistoury into the œdematous tissues at the base of the tongue. The operation is not a very serious one. You protect the blade of your bistoury with a piece of lint, and, taking the point of the knife under your forefinger, pass it well back to the epiglottis, puncturing and incising in the neighbourhood of the epiglottis and back of the tongue, and so giving very great relief to the patient. Some little blood comes away and a good deal of serum, and then, by assiduous hot gargling, very probably the patient is permanently relieved.

Of course, a case of this kind may at once be relieved and get well; but you must be prepared for a more serious proceeding if it should be necessary, and, as these cases generally get worse towards night, you should make a point of seeing them in the evening, and be prepared if necessary to do laryngotomy. I say laryngotomy advisedly, rather than tracheotomy, for this reason: the œdema never extends below the vocal cords, and if you do laryngotomy, you are, of course, completely below the vocal cords, and give your patient efficient relief, and with very little risk. Tracheotomy, of course, is a perfectly justifiable proceeding, and if you have got assistance handy and all your necessary instruments, I should not object to your doing tracheotomy if for any reason you preferred it; but I am supposing a young surgeon who is away from all assistance, who has to act on his own authority, and I strongly recommend him to do laryngotomy rather than tracheotomy. When laryngotomy is done, of course the ordinary laryngotomy-tube should be inserted; but if you have not got a laryngotomy-tube, a small tracheotomy-tube will answer every purpose; and even in the absence of all tubes, it is quite possible to keep the wound sufficiently open with a pair of scissors, or even with a safety-pin. In that way you may keep open the wound until you have had time to procure the ordinary tube, and the probability is that at the end of forty-eight hours all œdema will have subsided, the tube can be removed, and the patient will get perfectly well.

These, then, are the three forms of acute sore throat to which I wished to direct your attention. Let me say, now, a few words with regard to the more chronic forms of sore throat; and first with regard to that form which we know so well in hospitals as "hospital sore throat." I mean when a resident in the hospital, being necessarily exposed to bad air and hospital influences, finds that his throat is sore, that he cannot swallow quite comfortably, that he has lost his appetite, and that he is feeling generally ill. When you look into a throat of that kind, you see a general congestion of the parts, which, of course, it is important should be relieved as soon as possible. Now, for that kind of sore throat, which you may meet with also in private houses as the result very often of some bad air from drains, I know of no better local application than sulphurous acid and glycerine, in equal parts, which, painted freely on the fauces, will give immense relief. At the same time, the patient should be taken out of the evil influences from which he is suffering; should be sent into the fresh air, should have plenty of nourishing food and some wine, and he will very soon get well. Those are cases which recover most satisfactorily if the patient is put under proper hygienic conditions.

You may, however, have a patient come to you with sore throat which is due to a very different cause from that I have mentioned. A young man comes to you simply because his throat is sore, but when you look into it you see at once a very characteristic appearance on the tonsil, a grey excavated ulcer, which is always the result of secondary syphilis. Then, again, you may meet with this in a married woman, who has not the least suspicion that she has got disease from her husband, and I need hardly say how cautious you should be in giving the slightest hint which may lead to domestic unhappiness and be a breach of professional confidence. These cases of secondary syphilis, of course, are simply symptomatic of the syphilitic poison which is already in the patient's blood. It is no business of yours to find out very particularly how the poison got there; it is quite sufficient that you recognise a secondary syphilitic throat, and you proceed to treat it by treating the syphilis and also by treating the throat. The bichloride of mercury is, no doubt, the best remedy which can be given for secondary syphilis, and, as regards the throat, you may brush it over with any stimulating application. Perhaps a mercurial gargle is one of the best applications, a bichloride of mercury gargle, say one of a quarter of a grain to the ounce, with a little honey; and you let the patient gargle his throat two or three times a day with that until the ulceration is healed.

But there is another form of sore throat which you may see both in adults, and occasionally in children. I mean where you have tertiary or congenital syphilis affecting the palate and pharynx. Now, whenever you see ulceration of the back of the pharynx, always suspect that it is of syphilitic origin. In the great majority of cases you will be right. Patients who have no other symptoms whatever of syphilis have this tertiary ulceration of a peculiar serpiginous character, affecting the palate and pharynx, and children who have inherited congenital syphilis from their parents have the same kind of affection. Those are all cases which can be very well treated by iodide of potassium internally, and by local insufflation of iodoform, or, perhaps better, iodol combined with borax. The formula I generally use is one part of iodol to seven of borax, and I find that this blown into the throat is an extremely useful and satisfactory application.

Then, you will have before you from time to time cases of chronic tonsillitis, mostly occurring in children and young people. The appearance of a child who has enlarged tonsils is very characteristic, because he cannot breathe through his nose. He sits with his mouth open, which gives a peculiar and very characteristic heavy or stupid appearance to the face. Then, again, you will find that in many of these cases there is a certain amount of deafness, due to the involvement of the Eustachian tube, and many of these unfortunate children are thought to be very stupid when they really are a little deaf. I warn you, therefore, that you should take care to protect these deaf children from being treated as inattentive, when really the fact is they cannot hear.

When you look into the throat and see the large tonsil, there cannot be a doubt about the nature of the case. The tonsil is two or three times its proper size, and meets its fellow in the middle line. Not merely is the tonsil enlarged, but you will notice probably that there is a good deal of chronic affection of the whole of the mucous membrane of the pharynx and naso-pharynx; there are, in fact, adenoid vegetations which extend up into the posterior nares. Now, a tonsil, of course, can be treated by various local applications, but, after all, the best plan, I believe, is, in case of the chronic enlargement, to remove a portion of the gland. By far the simplest method is to use the ordinary guillotine, by no means a new instrument, which takes off a considerable portion of the tonsil, but does not aim at removing the whole. Having got rid of the tonsil, you will then proceed to treat the adenoid vegetations, which can be scraped away most satisfactorily with the finger if you gag your patient (and it is hardly safe to deal with a child without gagging it); then slip your finger behind the soft palate and up into the pharynx, where you will come to a mass of soft tissue filling up the posterior nares. With the nail you can scrape this away very satisfactorily, and more so, I think, than with any instrument. The growths being away, the patient will be able to blow the nose, and you can follow up the treatment afterwards by introducing a bent brush behind the soft palate into the naso-pharynx, and applying to it glycerine of carbolic acid. That applied thoroughly to the naso-pharynx produces extremely good effects, and the painfulness of the application can be completely got over by using a little cocaine with it.

The cases of chronic tonsillitis and adenoid disease occur in children of the strumous type, and you therefore must also treat the so-called struma. Many of them, no doubt, are instances of congenital syphilis, slight it may be, but still there is the syphilitic taint, and it would be well to bear that in mind, and to examine the condition of the teeth and to see whether there are any evidences in the central incisors of the notches, producing what are known as Hutchinson's teeth. In these cases cod-liver oil is extremely useful; the syrup of the iodide of iron is perhaps still better, or any of the ordinary tonics used in childhood may be employed. But, after all, the great thing is to get those children out into the fresh air as much as practicable, and if possible to send them to the sea-side.

Then we come lastly to a form of sore throat which has got a name given to it on account of the class of persons in whom it most commonly occurs—I mean *dysphonia clericorum*, the sore throat of the clergy. You will find that you are frequently consulted by young clerics because they suffer from constant hoarseness and sore throat. Now, one reason why they suffer in that way is because they have to use their voices, and they have never been properly taught how to speak. If the clergy would learn to use their lips and always to speak out to the man at the end of the church, the intervening members of the congregation would have no difficulty in hearing what they say. But if they will speak, as some of them do, in their throats instead of enunciating their words with their lips, of course they are not heard well, and there is a tendency to produce, from the strain upon the throat, a congested condition of that part. It is a curious thing that you are hardly ever consulted by elderly married clergymen for affections of this kind. It is in the young and unmarried that dysphonia clericorum occurs, and, for my own part, I must confess that I think it is a good deal a nervous disease, and that it is connected in some way with the sexual organs. When a man gets married he forgets about his voice and settles down to his clerical duties, and has no more difficulty; but it is the young and unmarried curate, who is no doubt often placed under circumstances of considerable trial, who finds that his voice is breaking down and that he is, consequently, obliged to throw up work and to seek medical advice. I am not at all an advocate of early marriages for the clergy, but still I am quite sure that a good many of these sore throats are cured, so to speak, by matrimony.

When he comes to you for advice, the first thing I should strongly urge you to do is to advise the clergyman to use his lips. If he will use his lips, as I said before, he will save his voice; and then, in addition to that, you may very well give him a spray to use. Before he goes on duty, before he goes into the pulpit, he should spray his throat; and, perhaps, one of the simplest and best sprays is a spray of sulphate of zinc, two grains to the ounce, which is perfectly harmless, and which I have known to be extremely useful. Then, again, he may do good by sucking a lozenge occasionally. Perhaps one of the best is a tablet made of one grain of chlorate of soda and three grains of borax. This is made into a tablet, and can be sucked slowly, and certainly gives the patient relief. In that way the case is to be met, and also by giving a tonic where it is required; but, as I said before, it is a troublesome complaint of young men, which, fortunately, tends to wear itself out.

ON

A CASE OF SUB-PERIOSTEAL RESECTION OF THE TIBIA.

I WANT to bring before you to-day a case of disease of bone which is very important, and particularly to you who are soon going into practice, because you may at any time meet with a similar case, and I am sorry to say that such cases are very often misunderstood, and therefore not properly treated. I refer to a case of acute periostitis of the tibia, or periostitis of a long bone, leading, as it generally does, to necrosis of that bone. I will now read the notes of the case—it is that of a boy on whom I operated last Wednesday—and then make some general observations upon the disease itself, and upon this particular example.

The boy's name is F. F., æt. 10, and the history of the case is this—" On the 4th of April, while he was minding some cows, one of them kicked him on the knee. He did not feel much of it until the next day, when he walked home quite lame, and the leg began to swell. He was seen by a local doctor, who made an incision on the inner surface of the leg rather below the middle, but there was no discharge of pus (?). The leg continued to get worse, so that the patient was unable to use it, and he was admitted into University College Hospital on the 10th of May. On admission the right leg was swollen, the lower two-thirds of the tibia were felt to be enlarged, and just below the middle of the inner surface was a sinus in the centre of a cicatrix, and through this rough bone was felt above and below the sinus. Below the sinus was a soft fluctuating swelling, and the skin was a little discoloured. Below the knee there were some cicatrices. The patient kept his knee flexed at right angles, but it was easily straightened." It was not noted, as it ought to have been, that there was a decided deformity of the leg, and the reason for this we found afterwards to be that there was a separation of the upper epiphysis from the shaft. The deformity was quite visible, but I did not know exactly the nature of it, for the simple reason that I did not care to pull the boy about until he was under chloroform, because it gave him great pain and he cried a good deal. When the boy was in the theatre on Wednesday I examined him a little more carefully than had been practicable before, and the result of his coming in here was that I removed the entire shaft of the tibia which you see in this bottle. I have also here a preparation brought from the museum, about which I shall have a word to say presently in illustration of this case.

You see this boy simply had a kick from a cow; I do not know what sort of a kick it was, but one would suppose it could not be anything like so severe as the kick of a horse, there being no iron on the hoof. The medical man who wrote to me about the case said that he was not called in to see it until some days after the injury, and then he found that the mischief had already occurred. He made the incision which you have recorded here, and matter was (according to him) let out. The accident was on the 4th of April and my operation on the 17th of May, showing that about six weeks was the period occupied in the entire course of the mischief.

Now what happened in this case I have no doubt was this. The boy is a fairly healthy lad, but the effect of the injury was to set up acute periostitis. He had doubtless inflammation of the periosteum, with all the symptoms of inflammation, pain, heat, redness, and swelling. The swelling never subsided, but it went on in the ordinary course to effusion beneath the periosteum, between it and the bone; that effusion rapidly became purulent; we had pus burrowing up and down the limb, stripping up the periosteum for a considerable distance, perhaps not the whole shaft at that time, but gradually extending up the shaft; and we had as the result necrosis. You could hardly imagine that in a case of this kind the first injury would be of sufficient moment to lacerate or damage the interior of the bone in any way; that is hardly conceivable. The bone was not broken by the force of the blow, therefore we may suppose that all the mischief was the result of periosteal inflammation. The case was not treated (except domestically) until the matter had already formed, and then very properly an incision was made, but that incision was too late to relieve the tension and prevent necrosis, for the bone was in fact already dead. After that a certain process of repair took place—limited repair—and it was in that condition that the boy was admitted here.

We have here before us a dried specimen and a wax model made from it in the recent condition (and the model is in every way better than the specimen) of a case of Mr. Marshall's, where he found it necessary to amputate the leg, and it serves exceedingly well to illustrate all the changes that I want briefly to run through. In the first place we have the stripping up of the periosteum —that of course is not shown here—and the destruction of those sub-periosteal layers of cells from which bone is formed. This boy being a growing lad there would be a rapid growth of bone taking place, and the layer of cells beneath the periosteum would be constantly laying down new bone; but these are destroyed by the inflammation, and the matter strips up the periosteum. Opinions of pathologists differ as to whether it is the entire periosteum or only a portion of it; but the result is the same. We have the bone thoroughly exposed and dying. You see in the specimen I show you some of the dead bone, it being in some parts perfectly white. It is one of the great characteristics of a necrosed long bone, that it looks like a piece of macerated bone. It is hard when you tap it, and does not bleed when you scrape or scratch it. You remember we saw that exceedingly well exemplified when the boy was on the operating table, for I called your attention to the white bone standing out between the granulations.

Fortunately, as soon as the mischief is done repair begins. The periosteum has very great vitality. You can hardly destroy it; so that although it is stripped up, still it has the power of pouring out osteo-genetic cells, and these cells surround the sequestrum and form new bone. You see in this museum-specimen the shell of new bone which we so commonly find, with an opening through which you can put the probe and feel the sequestrum inside, the sequestrum being of course the original bone that is dead. You see here a quantity of entirely new bone, exceedingly vascular, and laid down somewhat irregularly, having at this time the canaliculi and the lacunæ, and later on regularly formed Haversian systems. But supposing one is able, as one generally is in these cases, to get out the sequestrum, leaving the shell, then repair goes on. We have a filling up of the shell with granulations, in which new bone is formed, and a certain amount of absorption and shaping of the bone taking place, and eventually the formation of a canal inside, so that sometimes you can hardly tell the entirely new formation from the original bone on the opposite side. Of course there are cicatrices; but in a case that has done well the limb is as well made and strong as the other.

The explanation of the satisfactory repair of the bone in this specimen is of course that the necrosis, as a rule, simply affects the diaphysis or shaft of a long bone. Being from a young subject, we have at each end the epiphysis complete, therefore the knee-joint or ankle-joint would not be involved. That is exactly the case in the boy of whom I am speaking. He is only ten years old, therefore the epiphyses are still separate, and the disease was limited entirely to the diaphysis which you see in the bottle. I do not know the exact history of the case from which the model was taken, as I did not see it; but it has evidently been a case of acute necrosis with considerable repair which has been allowed to run the ordinary course, and then, unfortunately, the limb had to be amputated. I do not know why, but I presume because the patient's health had so run down that Mr. Marshall did not think it right to run the risk of further exhaustion on removing the sequestrum. We have in one of these other bottles a capital specimen of a large sequestrum of the tibia, with plenty of new bone all round it, which had been removed by amputation at some period and placed here for teaching purposes. Nowadays we seldom get specimens of this kind, because we prefer to cut out the sequestra, leaving the shell of new bone. Here, again, is another example which shows a very imperfect development of new bone. You see the sequestrum all bare and white, and at the lower and upper parts you can see new bone, but I do not know the history of the specimen.

Now let us see what occurred in the case before us. The whole of the shaft of the bone had died, and we see that in a very early stage the periosteal mischief had extended up the whole of the bone, and that both ends of the diaphysis were bathed in pus. Could we have left this case to nature and have allowed repair to be set up all round it? No doubt we could except for one drawback, namely, the excessive pain the child suffered; and there was also the fact that the epiphysis was already dislocated at the upper end. Then supposing I had chosen to leave it, would there have been any risk to the child? In the first place, there would have been great risk of exhaustion from the constant discharge of pus and the pain. Then, secondly, there is another considerable risk which one always has to bear in mind, that it is the constant suppuration going on, and the effect produced by that upon the system generally. You all know that form of disease called lardaceous disease of the liver and kidneys, which seems to be connected in children with long protracted suppuration. I believe we have a specimen of that disease in the child, the head of whose femur I removed the same day—the boy in No. 1 Ward, who, I am sorry to say, is not getting on very well. In that case I believe there is considerable lardaceous disease about the liver, and possibly about the kidneys, the result of the suppuration going on for many months or even years.*

What I did was this. I divided the periosteum, and extracted the sequestrum. That sequestrum differs much from the ordinary sequestra you may see here. I would particularly call your attention to this, that down the front of the bone you can see perfectly white and dead bone, and quite at the back you can see a piece added on to the shaft, that is really a piece of a shell of new bone which you see yourselves came away with the old shaft. This can hardly be called simply a sequestrum, because some of the bone is undoubtedly living; it is really a removal of the tibia, partly dead and partly living, the reason for the operation being that it was perfectly separated at both ends. Instead of being a case of removal of sequestrum, we may more properly call it a case of "sub-periosteal resection." This is an operation that has been a good deal talked about, but you do not often see it done. I believe that many cases of so-called sub-periosteal resection are nothing more than a pulling out of sequestra from a shell of new bone. This really was a case of sub-periosteal resection, the soft tissues (for you cannot separate one from the other) being turned back, and the whole length of the shaft being pulled out, bringing away with it some of the repairing new bone.

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* A post-mortem examination of the case of hip disease here referred to confirmed the diagnosis as to the existence of extensive lardaceous disease of the liver.

What will be the result of the case? Have I in any way spoiled the case by this proceeding? I believe not, I believe I have done the patient a great deal of good. Instead of having all that length of bone with suppuration going on down the shaft of the tibia, he has now got a large cavity, which you will see when we go round the wards; this large cavity is already beginning to granulate, and will fill up rapidly, so that there will practically be a new bone formed. Then will it be a better bone than one which he might have had if I had waited until the new bone had formed all round, running the risk of certain accidents which might have followed, and then pulled out the sequestrum? Well, I believe he will probably not have so good a limb. That point should be clearly understood. My own experience on the subject is limited, but I know that the experience of other surgeons is that there is a tendency in these cases for the limb to shorten. That is more particularly the case, of course, in a limb with only one bone. If you remove the humerus or the greater part of the femur sub-periosteally, you are almost certain to have a shortening of the bone by the contraction of the muscles round it. In this case, fortunately, we have the fibula acting conveniently as a kind of splint at the outside; therefore the shortening will not be great. We had a back splint ready also to put the limb upon at once after the operation, and it will be kept there carefully until the bone has become sufficiently thick to support the limb; still I am quite prepared to find that we shall have a little shortening on that side, partly from the shrinking and contraction of the muscles, and partly for the want of growth; for where we have taken out such a great piece it stands to reason that the limb will not grow, as the growth must be checked to a certain extent by operative interference, to say nothing of the injury, for injured bones do not grow so rapidly as the others. There were no measurements made between the limbs; it was impossible to do so; but I daresay at the end of six weeks after the accident we might have found already some little difference.

Now suppose, instead of doing that, I had left the sequestrum until the new bone had formed another shell round it, and then taken it out. The drawback to that is that you have a great deal of trouble in getting out the sequestrum. If you have a shell of new bone half an inch thick, or even thicker than that, formed of dense firm bone, it requires a great deal of trouble to open the shell so as to get the sequestrum out. Some of you may remember that last year, in a boy who came in with a large sequestrum of his femur, I had a great deal of trouble in removing it. I had to trephine the bone, opening up the shell to a great length, and even then I had great difficulty in getting the sequestrum out; it was, however, got out, and the boy did exceedingly well.

I think we now understand pretty well the present and future of the case. Let us go back for a minute or two and consider the early symptoms and the treatment at that stage. I want particularly to mention these, because you will see cases in their early condition of periostitis and commencing necrosis more often in private practice than you do in hospital; and the welfare of the patient will often depend upon the acuteness of your diagnosis and the treatment you adopt early in the case. It happens every now and then that one is called in to a case and finds that the bone affection has been overlooked. The patient has complained possibly of some swelling of the limb, which has been put down to rheumatism or what not. A child may be supposed to have fever because he gets into a low typhoid state, and when you are called in at a late stage, you find the limb full of matter, and on letting it out you find the bone necrosed, which is a very serious state of things. I have known more than one patient die in that way, in whom the disease was overlooked at first. It is therefore very important to recognise it early. In some cases of course there may be a very obscure injury, but in the majority of cases in children I think there is a history of some blow on the leg. The child may have been previously in perfectly good health, or it may have been strumous, but really there is no rule about that; it occurs as often in children who have been previously healthy as in weaker children. Then there is pain, heat, redness, and swelling of the limb and more or less constitutional disturbance, the child does not sleep well, it wakes and cries out, it loses its appetite, and it may have rigors on the occurrence of suppuration.

Now take the early stage before suppuration has occurred, can you stop it? I believe you can. I believe if you see the case quite early it is possible to stop suppuration occurring, and of course it is very important to do so. An early incision is the great thing in this case, and the reason is obvious. In the first place you divide all the dense parts over the bone, and you let out the blood from the superficial parts and the effusion from the deeper parts, that effusion between the periosteum and the bone which is likely to do so much damage by stripping up the periosteum. You may of course cut down upon the bone and find no pus, but then you must not be disheartened; you should never be too prophetic in your diagnosis and say there is pus, because the best of us may be deceived. There is nothing more deceptive than a small quantity of fluid deeply beneath the tissues; you may diagnose it, or you may not. But if you cut down upon the bone, mind and go right on it; divide the periosteum freely and firmly, until you feel the knife grating against the bone, and let out whatever effusion there is; if there is pus you will see it, if there is a small quantity you may overlook it, but whether there is pus or not that is the right treatment; it may relieve the case altogether, or it may not. You may find that the pus is still burrowing, and then the question is whether you will open it still further. I should strongly recommend you to do so. You should make a free incision as far as any swelling goes, you cannot do harm by it, and you may prevent it going further. The best thing that can happen is that you should get a superficial exfoliation. We have in this bottle specimens of a number of little exfoliations from the surface, the bone being pushed off by the granulations behind it. More commonly we get a sequestrum formed, either a limited sequestrum or one taking in the whole length of the bone.

Suppose, now, you get the whole bone exposed, how long will it take before you can safely pull it out? That will depend greatly upon how much repair has taken place. In our case we know exactly how long has elapsed, as we have the precise dates. It is as nearly as possible six weeks between the time of the injury and the removal of the bone by sub-periosteal resection, and that, I believe, is about the average time for a sequestrum to separate at each end from the epiphysis. In other cases, as in this museum-specimen, it will be many months before you get the bone loose, and that is really the great difficulty, that we have no positive guide to tell us when the bone is loose and when you can safely take it out. Some lay down the rule, and it is not a bad one, that you should not interfere in these cases until the shell of new bone has developed enough to support the limb. There is some truth in the doctrine, although it is not universally applicable, because it is extremely inconvenient if you get the new bone broken. If you pull out the sequestrum and leave a thin shell of bone around, it is possible that that shell may break, and then you will have the difficulty and annoyance of having to treat a fracture as well as a case of necrosis. I have seen that occur, and, unfortunately, it leads to bad results, that is, to amputation of the limb, because the patient's strength and the strength of the surrounding parts, the periosteum and so on, are not sufficient to throw out bone enough to repair both the fracture and the loss of bone.

Six weeks, I take it, is the earliest period at which you should interfere, and from that you may go on to any time you like; the difficulty, as I have said, is to be sure whether the bone is loose. It is all very well to say that you can put in a probe and feel. Theoretically, that is very easy, but practically, it is very difficult. You will easily understand that if a piece of bone is locked firmly into new bone you may poke about with the probe to any extent; you may feel the bare bone, and yet you cannot feel whether it is loose or not. Therefore one has to undertake an operation of this kind somewhat in doubt. If the sequestrum is loose, you can pull it out, if once you get fairly down to it. Again, you may make an incision and find it not loose, and you may leave it a little longer. You must all have seen examples of that kind. It is impossible, however skilful you may be, to be sure of your diagnosis in that respect.

So much, then, for the treatment. Just one word in regard to what will happen in the case of the patient upstairs. We have got now a shell of new bone more or less complete, opened up in the front; the back is a little spoiled, because we have brought away with the shaft of bone a piece of the shell of new bone; still, I was quite sure when I saw it on the table after the operation that there was sufficient new bone to set up repair if all went on well. What will happen is this. We shall have granulations springing up from the interior of the bone and from the soft tissues; these will gradually fill up the cavity; we shall have bone developed tolerably rapidly from the periosteum and the surrounding parts, and thus we shall have a new tibia formed. It will take many weeks to complete, and you will have an opportunity of watching it. If all goes on well, I have no doubt it will be complete; but one or two accidents may happen. The epiphyses may be involved, and it is not at all impossible that the child, after lying for some weeks in bed, and apparently doing well, may have acute mischief occurring either in the knee or in the ankle-joint. That would be due to perforation of the epiphysis taking place, the epiphysis not being very thick, particularly at the lower end. There is about a quarter of an inch at the lower end, or not so much, between the matter of the cavity and the ankle-joint; and it is within the bounds of possibility that, notwithstanding all care, some day or other the epiphysis and cartilage may be perforated, and we may find that mischief at the ankle-joint has set in, or the same thing may occur above, and then there is nothing to do but to amputate the limb. I do not know why perforation should occur more in this class of cases. No doubt these cases often do well, provided the patient's health holds out. Of course it will be a great strain upon his health at ten years of age to make all this new bone, and we shall therefore keep up his strength as much as possible. Then there is always the concurrent possibility of pyæmia, or of abdominal mischief which may happen in the case. Still, I hope he will do well, and that, barring the shortening which I have already explained, there will be a good result.

I must not forget to mention that this sort of treatment is quite modern. It is only within the last twenty or twenty-five years that this treatment of pulling out the sequestra and leaving a shell of bone has really come into vogue. Sub-periosteal resection, if you regard it as a separate operation, is still more recent, and only dates from 1857 or 1858; but many of the cases, as I have said, are not cases of sub-periosteal resection at all, but rather cases of pulling out ordinary sequestra.

Then there is another point which I ought to mention—I mean treatment by sulphuric acid. I mention it because it is one of the last inventions brought forward. There was a child in No. 5 Ward during the winter session, on whom I tried it. It was a case of necrosis of the tibia, in which I was able to take away some of the bone; but there was a piece of the sequestrum not loose, and I thought it would be a good case in which to try the sulphuric-acid treatment. I filled up the wound with a piece of lint dipped in sulphuric acid, one part of sulphuric acid to three or four parts of water. It was dressed carefully with the acid, but really I did not see that it made much difference. The theory is that the acid eats into the dead bone, and not into the living; that it destroys the dead bone, and either removes it altogether or facilitates the removal afterwards by the surgeon. We lost sight of the patient; but during the time she was in the hospital I did not see that there was much difference between her case and ordinary cases. That is all the experience of the sulphuric-acid treatment which I have had in the hospital, with the exception of that case of disease of the ankle-joint (a different class of case) which is still under observation, and in which I hope to get anchylosis of the joint. I do not think very favourably of the sulphuric-acid treatment; but clearly this case of which I have spoken to-day was not a case for trying anything of the kind.

ON

INTESTINAL OBSTRUCTION.

We have had in the wards lately a very interesting case of intestinal obstruction, which you all, I presume, saw; and on which some of you had the opportunity last Saturday of seeing me perform an operation. We succeeded, as you remember, in finding a piece of bowel twisted and bound down by a band. That I divided, and I hoped that the patient would be permanently relieved by that interference. Unfortunately, however, although she rallied from the operation very well and had a good night on Saturday, the bowels being freely relieved, early on Sunday morning, about nine o'clock, she suddenly collapsed, and died very shortly afterwards. After the lecture we will go to the post-mortem theatre and see what the condition of the parts is.

The patient was an unmarried woman, æt. 29. She was a shopwoman at one of our large drapery establishments; and the history was this:—She had been perfectly healthy before Whit-Monday, when she went into the country to see some friends. As far as she could tell she had not eaten anything to disagree with her. On Tuesday she was as well as usual, and had an action of the bowels; but after dinner she had a pain, which she described as having been of a twisting character. This came on while she was walking across the show-room, at about three o'clock. She then had a motion, but the pain was not relieved. She sat down, and went home rather earlier than usual. A little warm brandy and water was given to her, which she vomited about seven P.M. Then she had some opening medicine, which she also brought up. She was sick all night; and on Wednesday morning they had a doctor to see her. He gave her an injection, which did not open the bowels, but she passed some wind. On Saturday, the 26th of May, Dr. Ringer was sent for; he ordered an enema of castor oil and turpentine, and gave her opium. On Sunday the sickness had ceased. Dr. Ringer saw her again, and she was admitted here on the Monday, this day week. I did not see her myself until the Wednesday, as I was absent at the examination at the College of Surgeons. Mr. Beck saw her on Monday and Tuesday, and he put her upon opium treatment, giving frequent and small doses of the drug; and I found her taking that when I saw her on Wednesday.

I will now read you the account of what her condition was when she was first sent here:—“She is a well-developed young woman, with slightly anxious expression of countenance. Pale, with no pinching of the features; tongue dry, with slight brown fur; pulse regular, and normal in size and firmness. Temperature 98·8°. Belly moderately distended, no pain or tenderness; marked tympanitic percussion in each flank, and across the front of the belly; dulness over the pubes. No tumour to be felt, no hernia, no pain over the abdominal muscles or down the inner side of the thigh. An examination per rectum shows the rectum dilated, but no lumps of fæces; above the cervix uteri coils of intestines are felt in the pelvis, but no hard masses of any kind.” I need not trouble you with the daily notes of the case. I continued the treatment which had been judiciously begun, giving opium, and endeavouring to pass a long tube.

The symptoms gradually became more urgent, and the patient began to have stercoraceous vomiting. The first notice of stercoraceous vomiting is on the 30th, and then again upon the 31st; and on June 1st there is this note:—“The patient vomited stercoraceous matter again this morning. Mr. Heath gave her another enema of warm water, which came away as fast as it was injected. He then injected some olive oil, and this also came away nearly as fast as it was injected; but he thinks that a small quantity has remained." The report on June 2nd—that is, Saturday—was that on the previous afternoon, between three and four o'clock, she had vomited three-quarters of a pint of stercoraceous matter. She was also sick at nine and eleven o'clock at night, at 4 A.M., and again at 5.30 A.M.; so that when I visited her on the morning of June 2nd, I found that there had been five or six distinctly stercoraceous vomitings, following each other at intervals of two hours. That showed that the case was too urgent to be left any longer. We had been hoping all along that we might have nature producing a cure, as happens not unfrequently in these cases. Although the cases go on for some time without any action of the bowels, yet owing to some re-arrangement of the abdominal contents, perhaps some slipping back of a piece of intestine, suddenly the patient begins to be relieved of the contents of the bowels, and a cure succeeds. Still, when you find a patient like this vomiting frequently at intervals of about two hours, it is obvious that something must be done.

The operation which I did on Saturday was simply this:—I laid open the abdominal wall, about three inches below the umbilicus, just sufficiently to get my hand in. Then I found that the small intestines were enormously distended; they were of a dark colour, and considerably congested. On drawing them out we found that at one point there was a knuckle of intestine enclosed in a band, which band, I think, was not inflammatory, but was some little fold or piece of peritoneum; but we shall know more particularly presently. What I want you to understand is that the condition which existed was the strangulation of a knuckle of intestine by a band passing across it, the intestine above being largely distended, and the intestine below being very much contracted. I was afraid to use the knife to divide the band, and I simply tore it with my nail; I effectually tore it through, because I was able to squeeze a little of the contents of the intestine along this hitherto much contracted piece of bowel. Having done that, I was satisfied that I had relieved the stricture; still I thought it right to put my hand in and feel if there was anything more, because one knows that there may be two constrictions. Having enlarged the wound a little, I could put in my hand more freely, but I found nothing requiring interference. I felt the ovary and the broad ligament, and I found the whole of the pelvis was clear, and I therefore closed the wound. I treated the patient on the same principles as those on which you would treat a case of ovariotomy—that is, I closed the wound, bringing the edges of the peritoneum into close apposition, so that they might unite rapidly. I had hoped that the patient, as she had got over the first shock, would make a good recovery; that, however, has not been the case, and we shall soon ascertain the exact cause of death.

I will now say a few words generally about these cases of intestinal obstruction, which are the most anxious cases you can possibly have. Let me beg you to distinguish altogether these cases from cases of strangulated hernia, for I do not propose to speak of strangulated hernia at all. The causes of intestinal obstruction are various. I have here a table in a very valuable little book on intestinal obstruction, by the late Dr. Brinton. He gives a table of a large number of cases, and I will give you the details very shortly. He has collected the records of 12,000 cases of post-mortem examinations, and he finds that one in every 280 of these was due to intestinal obstruction. That will give you an idea of the frequency of the occurrence of these cases. With regard to the varieties, he found that intussusceptions were about 43 per cent.; external bands, that is, bands of peritoneum, or bands of lymph, as the case may be, 32 per cent.; parietal strictures, that is, strictures of the wall of the bowel, 17 per cent.; and torsions or twisting of the bowel 8 per cent.

With regard to cases of intussusception, which are much more numerous than the others, nearly half of the whole, 56 per cent., were ileo-cæcal, that is, the ileum being driven into the cæcum; twenty-eight were ileac, four jejunal, and twelve colic. The "ileac" means one piece of ileum driven into another piece of ileum; so with "jejunal" and "colic." More than half the cases you see are ileo-cæcal. The remaining forms of intestinal obstruction Dr. Brinton has divided into "bands and adhesions;" "diverticula ilei," where a little diverticulum has existed during life and then at some period has become attached, and so formed a constriction; "vermiform appendix," which occasionally gets bound down and forms a ring through which a piece of bowel slides; "ruptured mesentery," through which the bowel slips, as I fancy it has done in this case; and other peritoneal lesions. Then there is another form of obstruction occasionally met with, namely that of gall stones. Now and then a large gall stone ulcerates its way through the gall bladder—it is too large to go down the duct—and it ulcerates through the adhesions into the intestine. It is a rare thing, but it is right that it should be mentioned. The above forms of intestinal obstruction occur in the small intestines; but if you take the large intestines you meet with strictures and torsions there also.

But there is another division still more important. I mean whether the obstruction is acute or chronic. This was clearly a case of acute obstruction, but we have had the opportunity of seeing here within the last year or two several cases of chronic obstruction. We had one just before Christmas, in which a cancerous bowel had opened into the bladder. I performed colotomy there with great relief to the patient, and we have had other cases also in which colotomy has been successfully performed for chronic obstruction of the bowel due to cancerous or other stricture. But this was a case of acute obstruction. The young woman was in perfectly good health till suddenly, without any obvious cause, she gets pain in her bowel. She simply empties the bowel below the point of obstruction, and afterwards she passes nothing; then sickness comes on and the other symptoms which I have detailed.

The greater number of such cases are cases of intussusception, and these arise more frequently in children than in grown-up people. You find that when children are trying violently to empty the bowels they are very apt to force one piece of bowel into another, and then of course you get the symptoms due to that obstruction. There is generally a well-marked elongated tumour to be felt through the abdominal wall, which of course varies in length according to the amount of the intussusception. You see by figures 18 and 19, which by the kindness of Dr. Buzzard I have been able to borrow from Dr. Brinton's work, what the nature of intussusception is. The two mucous surfaces are brought almost into apposition, so that nothing can pass through; but the patient will go on straining and bring away a little blood. It is a remarkable point, and a very important diagnostic one, that in cases of intussusception you almost invariably have some blood pass, and you may see at once why blood should pass. Here is a piece of bowel nipped in another piece of bowel. Of course it becomes much congested; naturally the vessels relieve themselves a little, and blood passes on through the anus, and is recognised in the motions.

FIG. 18.

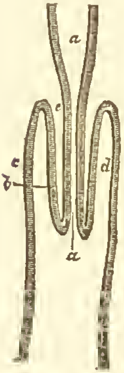
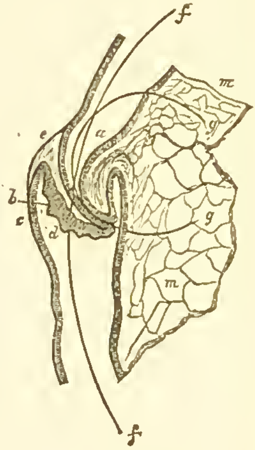


Diagram to illustrate the Intussusception of a Flexible Tube—(*a*) inner layer; (*b*) middle layer; (*c*) outer layer; (*d*) interval of two inner (mucous) surfaces: (*e*) interval of two outer (serous) surfaces.

FIG. 19.



Intussusception of small Intestine.—*A* to *e* as in Fig. 18; (*e*) being occupied by a mass of lymph; (*f*) general curve of the intestine; (*g*) special curve of the intussusception; (*m*, *m*.) mesentery.

Now it is possible that the intussusception may be undone. There is no doubt that the tendency of the peristalsis of the bowel is to force one piece of bowel further on; but if you can stop the peristalsis by giving opium judiciously, it is possible that the bowel may release itself, or that you may assist nature by means properly applied. The means we apply in cases of this kind, particularly in the large intestine, are copious enemata or insufflation. It was possible that it might be intussusception in the case of this woman, and therefore I was anxious to pass copious enemata into the colon, but we failed, and I think we shall find we failed simply owing to some mechanical condition of the bowel, and not from any obstruction of the large intestine—that owing to the distension of the abdomen there was some little twist in the sigmoid flexure, or the upper part of the rectum, and that that prevented the tube passing. When you have intussusception, then you stop the peristalsis by opium, and you may, if you are fortunate, blow out the invaginated portion, and so restore the intestine to its proper form; or you may, as a last resort, open the abdomen, and pull one piece out of the other.

There is, however, always great risk about that proceeding, and in this way. You must bear in mind that we have got two peritoneal surfaces here which are brought into apposition, squeezed tightly together, so that you are very apt to get adhesions forming between them. Imagine for a moment that you have those surfaces tightly bound together; it is then obvious that no pulling and no pushing can undo them with safety to the patient. Still you must not despair, even in a case of that kind, for nature does wonderful things for us. We find that cases of intussusception sometimes recover after casting off a piece of intestine. Thus, a patient has been apparently at death's door when at last something passes, or perhaps partially protrudes through the anus and is drawn away by the surgeon's finger, and when he examines it he finds that it is really a piece of intestine. I have myself seen a great part of the cæcum come away in that manner, in a case of invagination in a child, and the patient recovered. What happens is this: the adhesions are formed, the strangulation still continues, and at last, the parts being perfectly united together, the invaginated portion is thrown off, and the continuity of the intestine is kept up; the patient recovers, and does not seem to be much the worse for it, being only minus two or three inches of intestine. It is the knowledge of the fact that intussusceptions are very apt to get well of themselves, either by spontaneously undoing or by throwing off the slough of the intestine, that makes medical practitioners sometimes loth to interfere in these cases.

It is only within the last two or three years that surgeons have ventured to interfere in cases of intussusception, and I am happy to give credit—where it is due—to Mr. Jonathan Hutchinson, of the London Hospital, for having been, as I believe, the first surgeon who really did undo an intussusception. He has published his case, and it was a successful one, and I will briefly refer to it. On the 11th of November 1873, Mr. Hutchinson read a paper before the Royal Medical and Chirurgical Society, entitled "A Successful Case of Abdominal Section for Intussusception, with remarks on this and other methods of treatment." The patient was a child, æt. 2 years; the intussusception had commenced at the cæcum, and was of such a length that its extremity, presenting the inverted ileo-cæcal valve, was extruded several inches at the child's anus. The condition had been one month in course of development; latterly the case had been treated as one of prolapsus, and attempts had been made to keep the bowel in its place by means of a cork pad. It is obvious from these notes that it was not an urgent case. No one could have been so ignorant as to mistake a case of real intestinal obstruction for one of prolapsus ani. The child was very ill, and the author having failed in attempts to effect reduction by enemata, &c., and having had experience of several similar cases which had ended fatally, determined to operate. The child was put under chloroform, and the abdomen was opened in the middle line below the umbilicus. The intussusception was easily found, and as easily reduced. The after-treatment consisted only in the administration of a few mild opiates, and the child recovered.

That case was a very important one, because it cast a new light on the treatment of intussusception. There have been one or two other cases of a similar character in which surgeons have operated, some with success and some without success. At the Pathological Society in 1874 Mr. Hutchinson showed a specimen, this time from one of the lower animals, for of course they are liable to intussusception as well as ourselves. It was the case of a dog which had intussusception. He had only slight symptoms eight days before death. There was jaundice, no peritonitis, and the intussusception was easily reducible. It was operated upon to illustrate the possibility of drawing out intussusception. One or two other cases are mentioned by other persons; but the best cases were those related at the Medical and Chirurgical Society in December 1875. Mr. Howard Marsh read a case in which abdominal section was performed for intussusception in the case of an infant three months old. Another paper was also read by Dr. Fagge and Mr. Lucas on a case of intussusception in an adult, without symptoms of strangulation, treated successfully by abdominal section. A third paper was contributed by Mr. Jonathan Hutchinson on a second case of abdominal section for intussusception into the colon, with details of the operation. That case was not successful. A discussion followed, and opinions were greatly divided, some thinking that operations were perfectly justifiable, and others for the reason that I have given to you—the well-known fact that intussusceptions do recover—thinking that surgical interference was not justifiable. I think it only right that you should have these facts put before you.

Ours, as I have said, was not a case of intussusception; but before we leave the subject let us just consider one point in connection with it—that is, the fæcal vomiting which occurs in these cases sooner or later. It was maintained up to within the last few years that fæcal vomiting depended upon inverted peristalsis of the bowel. You will find that in some of the older physiological books it is laid down that, after the bowel has become distended, it is very apt to take on an inverted peristaltic action, that instead of forcing its contents on, it begins to push them backwards and throw them into the stomach, and so out through the mouth. Dr. Brinton, the author of this little book on intestinal obstruction, showed, I believe conclusively, that that view was erroneous; and it is now well understood what is the reason that in a case of intestinal obstruction from any cause you get vomiting of the contents of the bowel. He illustrates his view in this way; and I cannot show you better than by his own diagrams. Suppose you take a syringe, the piston of which is hollow (Fig. 20), and as you drive the piston down and bring pressure to bear upon the fluid at the side of the tube, you necessarily make the fluid return up the centre of the tube, the same thing occurring if there are a series of hollow septa, as in Fig. 21. He calls this a "reversed axial current," and it is that; the fluid goes down the side and up the middle, for it cannot stop where it is because the pressure is too great above it. There is another drawing of Dr. Brinton's which illustrates it in another way. It is a diagram of a piece of intestine (Fig. 22), and there is a stricture, which may be artificial or otherwise, for these things have been verified by experiment. If you

FIG. 20.

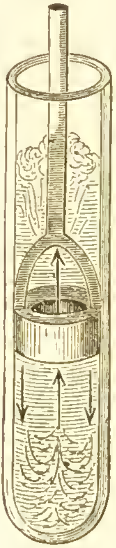
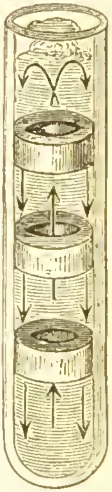
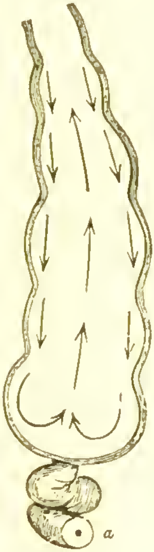


FIG. 21.



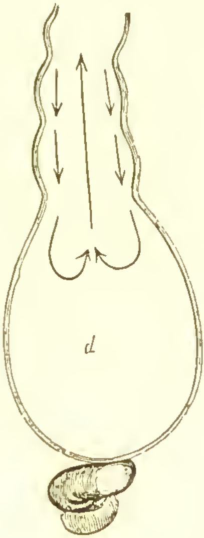
tie the piece of intestine and force the fluid in by a syringe, you will find that as the fluid goes down the side it comes up the middle again; but when you have a period of great distension, which you must necessarily arrive at, you may get a piece of intestine so largely distended that the peristaltic power in it is entirely destroyed, the muscular fibre is paralysed for a time, and then it simply remains an inert bag of fluid (Fig. 23). If that is

FIG. 22.



Stage of moderate distension, with forward and backward currents—(*a*) contracted intestine below the obstacle.

FIG. 23.



Stage of extreme distension in which (*d*) the dilated intestine is paralysed.

the case, you will find that the intestine above that point which still retains its power goes on pumping into this bag of fluid, and of course the same fluid that goes down comes up again. I want you to understand that it does not necessarily imply that the intestine keeps itself empty because the patient vomits. You may have a large accumulation, a piece of intestine enormously dilated and distended with fluid, and yet the patient may go on vomiting because peristalsis forces more fluid down which must come back.

I will now say a word with regard to the different forms of band. I have already given you Dr. Brinton's list of obstructions, but I will refer to it again for a moment. A band may be a simple adhesion, that is, composed of lymph. We know that all persons, females particularly, are liable to have a little attack of peritonitis, which apparently does no harm, but which may leave a little band stretching across between two pieces of intestine, or possibly stretching between the intestine and the abdominal wall. A piece of intestine may get twisted over this, and then you may have an obstruction. Then we have diverticula from the ileum. I do not know whether you remember that in the early stage of the fœtus there is a communication between the membranes of the fœtus and the small intestine by the omphalo-mesenteric duct which passes across from the umbilicus to the lower part of the ileum. That is usually totally obliterated, and we do not find even a remnant of it. Every now and then—I do not know why—we find that it remains as a cord, and sometimes even as a duct. I have myself seen it as a duct, so that the patient was annoyed occasionally by the passage of a little fæcal matter and flatus through the umbilicus. Usually it is simply an impervious cord. Now, given a cord of that kind, stretching across to the umbilicus, it is conceivable that a piece of intestine may get twisted around it, and you will find in Guy's Hospital two specimens of a cord stretching in that way, which proved fatal by a piece of intestine getting entangled around it.

Then the vermiform appendix may sometimes be bound down in such a way as to form a constricting point. Usually in the human subject the vermiform appendix is a simple matter that gives no trouble unless a cherry-stone gets into it; though Sir William Jenner maintains that cherry-stones do not get into it, and that what are generally taken for cherry-stones are little concretions. We do not usually know much about our vermiform appendices unless they give us some trouble, but if it is longer than usual it gets bound down and forms a ring, through which a coil of intestine may go, and of course it would nip it severely.

Then we may have a ruptured mesentery. Why a mesentery should get torn without violence we do not know, but sometimes holes seem to form in the mesentery, and a piece of intestine may slip through. I think ours is a case of that kind, from what I can see. I think the band I divided was not lymph nor the result of inflammatory action. The other conditions are of course inflammatory, and we have no evidence whatever of inflammation in this case. On Saturday I called attention to the fact that, although the intestines were largely congested, still the surfaces were perfectly smooth, showing that there had been no recent peritonitis as far as we could see, and that augured well for the patient.

Now to come to the really important point, that is, with regard to the treatment. If you are called to a patient who has intestinal obstruction, what is the proper line of treatment to be adopted? The great thing to make up your mind to early in life is not to use purgatives. What does a purgative do? In a case of intussusception it simply means driving more intestine into the other piece or if a piece of intestine is strangulated by a band, you simply drive more intestine tightly into it; therefore, when you have a case of intestinal obstruction—I do not refer to the case of every old woman who has overloaded bowels—purgatives should be tabooed. The proper treatment is to use opium. In this particular case you remember

I combined opium and belladonna. I said at the bedside that perhaps some might think I was firing off a double-barrelled gun, and that one might be antagonistic to the other. I did not know at the time that Dr. Brinton used the same form of treatment, but I will now read to you what he says, because he puts it in a shorter way than I could do. "The combination of belladonna with opium seems in no way to diminish the anodyne effect of the latter drug, even while it reduces this peristalsis almost to zero. Certainly nothing in the whole action of remedies can surpass the comparative painlessness into which I have seen an agonising intussusception converted during the many days that have intervened before its unavoidable termination in death." Then in a note he says: "I am aware that the two drugs are often supposed to be antidotes to each other. But though in stating facts I am not bound to explain them, I may point out that whatever general antagonism might be supposed to constitute the sum of special opposition in their effects is quite compatible with a general agreement of the most valuable kind, so that, for example, they might agree in their anodyne property, and differ mainly in their influence on the nerves of muscles or on muscles themselves. In any case it is impossible to doubt experiences which, like the above at least, prove that, mixed in certain proportions neither drug loses much, if any, of its most important effects. The proportions I use are two parts of extract of opium to one, two-thirds or one-half of belladonna in a pill." It is better to give pills than draughts in these cases. You know whether a patient rejects a pill or not; you can see it come back, and you can get a fairer notion of how much of the drug the patient is getting. Then pills get further down the intestines before they dissolve, and you get the effect of the opium more applied to the intestine itself, and not scattered over the stomach. You may give a grain every four hours, or at first as often as every two hours, and then reduce the dose, giving half-a-grain every two hours, and then half-a-grain every four hours, so as to keep the patient well under the influence of the opium, judging by the pupil and also by the general condition. Of course it would be as bad practice to poison the patient with opium as to give purgatives. I cannot say more than that; watch the condition of the patient and be guided by the symptoms. We applied belladonna over the belly, and assiduously poulticed it. The injections were an entire failure, and we shall now have an opportunity of seeing what the failure was due to.

The post-mortem examination showed that the obstruction had been completely relieved, and that it had been caused by a mesenteric band. The intestine was much discoloured at the point strangulated, but no perforation had taken place. Considerable peritonitis was found, and the intestines were glued together by recent lymph. Nothing abnormal was found in the rectum or elsewhere.

ON

DISLOCATION OF THE FEMUR.

You saw last week, gentlemen, a case of dislocation of the femur. Now, these cases are not, of course, very common; but it happens that we have had a good many during the last twenty years. I have taken the trouble to run through the reports of this hospital for that period, and I find that during that time we have had seventeen cases. We had one in 1871 (I need not trouble you with the details); two in 1872; one in 1873; one in 1876; two in 1879—and one of these was a supra-pubic dislocation, which, of course, is the rarest form; two in 1878; three in 1880; one in 1883; one in 1885; one in 1887; one in 1888; one in 1889; and none in 1890. It is rather remarkable that the last four cases were all in young persons. In 1885 there was a child of 12 years only; in 1887 the child was 11¾; in 1888 the child was but 6; in 1889 only 5. You may take it from me that these were all *bona-fide* dislocations, the result of accident, and not the form of dislocation which we call pathological. Of course, we are all familiar with cases of hip disease, where the capsule softens and the head of the bone slips out, and these we term "pathological dislocations"; but I am not speaking of that subject to-day. All the cases I am going to mention to-day are cases resulting from accident.

The man now in the ward is 33 years of age, and a labourer. Fifteen weeks before his admission here he had an accident. In jumping off a two-wheeled cart, which he had been driving, and in front of which he had been sitting with his feet upon the shaft, his foot caught in the horse's nosebag, which was swinging from the shaft, and he fell to the ground,—in what position, however, he does not know. He still held the reins, and stopped the horse. He says the wheel ran up his leg, but did not run over him; but he is not at all clear on the point. He was taken to a doctor within ten minutes of the accident, and the treatment employed consisted in rest, liniments, and so on. About six weeks afterwards he got out of bed, using first a pair of crutches and later on two sticks, and began to get about pretty fairly. But then, I suppose, the doctor's conscience smote him, and he thought he had better, perhaps, send the man to the hospital. When he arrived here there could not be a doubt, I should say, to even the meanest capacity, that it was a *bona-fide* dislocation of the femur on the dorsum ilii. The next day I sent the man down for the F.R.C.S. examination, and on the following day, the Wednesday, I had him photographed, and here you see the result. The front view shows you very well the deformity. You will see that the left limb is inverted, that it is shortened, and that the ball of the great toe rests on the instep or thereabouts. The inversion of the limb is undoubted. If anything, there is a little widening of the pelvis, the head being on the dorsum ilii and not in the sciatic notch, the prominence of the trochanter is a little exaggerated. If you have a case where the head of the bone has dropped into the sciatic notch, then you sometimes get remarkable narrowing of the pelvis. I had a side view also taken; but I am rather disappointed with that, it does not show the deformity caused both by the trochanter and the head of the bone as well as it should do.

How is it this dislocation was overlooked? I really cannot explain it. I tried in every way to get from the man that the parts were a good deal swollen before the doctor saw him; but it was not the fact. It is evident, from the notes, that the medical man saw him ten minutes after the accident, there could not, therefore, be much swelling; and it is obvious that it is one of those cases which have been overlooked, and which from time to time we meet with. We all make mistakes, and therefore we ought all to be charitable to others. I have purposely never inquired who the gentleman was that saw the case.

A word or two about the frequency of this dislocation. Malgaigne gives 491 cases of dislocation in the Hôtel Dieu, the great hospital in Paris. Of these dislocations 321 were of the shoulder and only 34 of the hip; but the hip was the next most frequent to the shoulder. Mr. Henry Morris in his article in Holmes's *System of Surgery* gives the statistics of Middlesex Hospital for ten years, and I may say, that these include both in- and out-patients. The statistics of this hospital, as given in our reports, only include in-patients; but I take it that there are very few cases of dislocation of the femur among the out-patients. Dislocation of the shoulder is, of course, commonly enough treated in the out-patient room; but all cases of dislocation of the femur are taken in as a matter of course. At the Middlesex Hospital, where both kinds of patients are taken together in the statistics, there were in ten years 283 dislocations, of which 133 were of the shoulder, and only eight of the hip; so that, you see, the proportion is very much the same in different hospitals.

As to the method by which a dislocation is produced there has been a great deal of discussion.

I do not know that the point is absolutely settled yet. Mr. Morris, the gentleman I have before alluded to, wrote a paper in the *Medico-Chirurgical Transactions* of the year 1878, in which he discussed this question at some length, and gave the result of some experimental inquiries which he had made. I give you shortly the conclusions he came to. The capsule of the hip joint, he said, is, as we know, weakest behind, and, possibly, below; and he particularly insisted upon the fact that the perforation was always at *the lower part behind;* that the bone then slipped up upon the dorsum ilii; and that therefore a dislocation on to the dorsum ilii is what is called a secondary dislocation. That is a view which has been a good deal discussed; and Mr. Eve, who was at that time Registrar at St. Bartholomew's Hospital, three years afterwards published a case, and contradicted Mr. Morris. It is the pleasant way we have, that one surgeon usually contradicts another. The value of Mr. Eve's paper particularly is that he gives drawings from actual specimens, which I have here, and which even at a distance I think you can see. Of course in these cases the patients either died at the time of or soon after the injury, and opportunity has been taken to make the dissection. We have also here from the museum of University College a specimen of which I do not know the history, as it did not occur in my wards.

In it you see the head of the bone; the quadratus femoris; the obturator externus torn, and the obturator internus stretched over the head. It is a dislocation, therefore, "below the tendon." If all the dislocations take place, according to Mr. Morris's suggestion, through the lower part of the capsule, it seems odd that so many of them should find their way afterwards up to the dorsum ilii; but the fact is that there are undoubtedly many cases—and I believe our patient's case was one of them—where the dislocation takes place directly backwards through the posterior part of the capsule, of course, but *through its upper part*, tears through some fibres, no doubt, of the gluteus medius and minimus, and comes out "above the tendon," as Bigelow would call it.

This, then, was a case of dorsal dislocation, or dislocation above the tendon; and it is my belief that it was a direct dislocation. Of course, fortunately, we have had no opportunity of a post-mortem examination, and we shall not have one. But Mr. Morris would argue, True it was a dorsal dislocation; but first it was a dislocation backwards below the tendon, and then, as a result of the man walking about, and so on, the head of the bone was pushed up, and it became a dorsal dislocation. Well, let it be how it may, there is no question that there are two dorsal dislocations which we have to recognise. One is that in which the limb is inverted and very considerably shortened, the toes rest on the instep, and, the trochanter being more prominent than usual, the head of the bone can be felt on the dorsum ilii, as in this patient. This is what Sir Astley Cooper called the *dislocation upwards,* or the *dislocation on the dorsum ilii.* It is now the fashion to call it *the dislocation above the obturator tendon;* but it is the same thing. In addition we have the second form of *backward dislocation,* where the limb is not much shortened, where the great toe rests against the opposite great toe; where the limb is more inverted but not so much flexed, and where the trochanter, instead of being prominent, is apt to be depressed; and therefore we have narrowing instead of widening of the pelvis. In addition, in the *sciatic dislocation* there is usually a good deal of pain down the limb from pressure on the sciatic nerve.

To finish the subject, although we have not a case at present, I would remind you of the other two forms of *forward* dislocation—forward and downwards *into the obturator foramen,* where the patient's limb is advanced and his body flexed; and, the rarest of all, the *dislocation on to the pubes,* where the limb is abducted and everted, and you feel the bone actually on the pubes close to the pectineus muscle.

We will go back for one moment to the anatomy of the hip-joint; and I have here a preparation to show the capsule. Here we have the posterior and lower part of the capsule, which is the thinner part; there we have the anterior part of the capsule, which is very much thicker, and remember that it has in front of it the great psoas and iliacus muscles supporting it; and it is this part on which Bigelow has laid so much stress, and which he has called by a separate name, viz., *the inverted* Y-*ligament*. I have Bigelow's book here, and will show you his own diagram, which no doubt is perfectly familiar to you all, as it has been copied into other books. It shows very prettily the two portions of the anterior part of the capsule, which is dignified by the name of the Y-ligament. We have in all anatomical books for years described an ilio-femoral ligament, and we have here another slip of it which really does not always exist. It is an exaggeration to say that in all cases you can make out a ligament like that, though you can in favourable subjects. However, the main point is this, that the anterior part of the capsular ligament is the strong part, the posterior part is the loose and weak part; and, whenever a dislocation happens, the head of the femur passes much more frequently through the posterior part, indeed some people go so far as to doubt if it ever comes through the front. Remember, too, that we have the ligamentum teres inside attached to the bottom of the acetabular cavity and to the head of the femur. Deepening the cavity we have the cotyloid ligament which maintains the head of the bone in the socket.

How then does the dislocation occur? (I am speaking now only of the dorsal dislocation.) Mr. Morris, in the paper to which I have already referred, holds very strongly that it always occurs in the position of abduction of the thigh. He says that if you have the limb abducted, a very little force will drive the head of the bone through the lower and posterior part of the capsule (the weak part), and that then being once dislocated it may be pushed up to any point, either lodging in the sacro-sciatic foramen, or coming up on to the dorsum ilii. Now, I have seen a good many experiments made at different times on this point, and I must say I am not at all convinced of the truth of that statement. If in the dissecting room or post-mortem room you want to dislocate the femur, the simplest way is to cross the thigh well over to the opposite side, which drives the head of the bone a little bit out, and then forcibly to jerk it, when out it comes. I have done this and seen it done many times, and I have not the slightest doubt that that is how the accident generally happens. Think for a moment. When the limb is well adducted and crossed over the opposite limb, already the head of the bone is a little bit out of the socket. A violent jerk breaks the ligamentum teres, drives the head of the bone either through the capsule or tears away the capsule sufficiently to let it out, and there is the dislocation produced.

Now comes the question of reduction. This accident had happened fifteen weeks before the man came here. 105 days had elapsed—in fact, the reduction took place on the 106th day. That is a long time for a bone to be out of its socket, and yet to be got back. But on looking up the literature of the subject I find there are cases of longer standing than that. There are cases on record—and I believe true ones—where even a year has elapsed and yet reduction has taken place. Finding that it was not quite a third of a year, I thought I might very well venture to try in this case, and we were successful. I think all surgeons in the present day are agreed that the manipulation treatment is better than the extension. Bigelow, who investigated the subject extensively, and whose book I have just shown you, has strong views upon it, and, I think, correct ones. His view is this: that what you want to do is to flex the limb and to adduct it with the object of relaxing, in the first place, the strong Y-ligament; then, having done this, that you should lift up the limb, bringing it upwards to a right angle with the abdomen, then circumducting it outwards you bring it down so as to get the head of the bone into the socket.

I had a good many difficulties to contend with last Wednesday, one being that the limb was fixed in its new position. I did not know exactly what the condition of things might be, but evidently there was a beginning of something like a new joint; and I had, therefore, to work very hard at first to break down the adhesions, and get the limb to move at all. However, after a time, we were able to flex it and adduct it; then to abduct it and rotate outwards and bring the limb down. I repeated that two or three times, but I did not get the head of the bone in. Mr. Pollard helped me, and we were getting, apparently, no nearer success when, as the limb was everted, Mr. Pollard gave it a slight tilt upwards and the head of the bone slipped in, and is now in its proper position.

The theory of the manipulation treatment is, that the ligament being the fulcrum and the femur the lever, you should adduct and flex and lift up; then circumduct outwards and bring the limb down parallel to its fellow; and if you are lucky the head of the bone will slip through the rent and into its place. Of course, after fifteen weeks, the rent must have somewhat repaired; and it is extremely probable, therefore, that I had more difficulty in that way because of the repair; but still the bone went in with such an audible snap and so cleanly, and remains so thoroughly in its place, that I feel sure there is no fracture, and that the head of the bone slipped through the capsule into its proper position.

In an interval of trying manipulation I thought I would give a demonstration how to apply extension; so, putting a wet bandage round the lower part of the thigh, and over that a skein of worsted made up into a clove-hitch, with my foot against the man's perinæum in order to get a fulcrum, I pulled, but did not produce the least effect. I hardly expected I should, but I wanted to show you how the thing could be done.

In former years I have reduced more than one recent dislocation of the femur in that way when the parts were all relaxed, and in a recent case you can pull well down and the bone goes in pretty easily. But I think the method of circumduction is better on every ground; still, it is right to warn you that accidents have happened with it. In an elderly patient the femur has broken, and that would be a very awkward complication. In this case I was careful what I was about. I felt a certain amount of giving of tissue; but that was merely the adhesions between the head of the bone and the dorsum ilii. I was careful not to put on too much force, because I might have broken the femur, and, no doubt, as it was, I necessarily lacerated some of the muscles of the thigh, and possibly tore some of the ligamentous structures too. However, "all's well that ends well." I am keeping the man in bed with a long splint on. I took it off just now and found everything in its place, and I have no doubt he will make a good recovery.

Just a word or two about the other form of *dislocation into the sciatic notch.* If it were a dislocation backwards below the tendon, the head of the bone would lie somewhere about the sciatic notch, although not necessarily actually in the notch. It comes back and presses upon the tissues which come through the great sciatic notch, or it may get impinged upon the spine of the ischium, or lodge on the lesser sciatic notch. When it really gets into the sciatic notch there is flattening of the buttocks. Another diagnostic symptom, useful to remember, is that if you put your finger up the rectum you can feel the head of the bone actually in the sacro-sciatic notch. In doubtful cases, therefore, it is well to have recourse to that method. If you think it is a case of sacro-sciatic dislocation the method of reduction will be exactly the same; you would adduct, flex, lift up, abduct and rotate outwards, and then bring the limb down.

So much for these two kinds, and now one word. to complete the subject, about the other two forms of dislocation. I show you the diagram of the *dislocation into the obturator foramen,* with the limb advanced and the body somewhat flexed; and, lastly, that of *dislocation above the pubes*, with the limb everted and somewhat flexed. Now, in these two dislocations forward, of course the head of the bone will come somewhat in the position that is represented there, that is to say, it will slip past the Y-ligament and through the anterior part of the capsule. We had some years ago in Mr. Marshall's ward a woman who had fallen from the top of a house, and dislocated her femur. The limb was inverted, and attempts were made before admission to reduce the dislocation. (This is her account, at least.) She was not admitted here until the sixth day, at which time the limb was everted and two inches short. It was reduced. In 1880 we had a case, also in a woman, who fell 60 feet. Her limb was everted, half an inch short, with the head of the bone on the pubes. The dislocation was reduced by rotation inwards and flexion. I mention the two cases because they are of different kinds. Because the limb was everted it did not show that the earlier one was a dislocation on to the pubes. It was six days old and had been well pulled about. It was really a dorsal dislocation, in which the parts had been so torn by the treatment before she came in, that you could move the limb pretty well anywhere you liked.

But the case in 1880 was a *bona-fide* dislocation on to the pubes, in a young woman of 23. I saw it with Mr. Marshall; and it is the only case of dislocation on to the pubes I have ever seen. It is so rare that one does not see above one or two in the course of a life-time. The reduction of these two dislocations is simple enough to remember. It is just the reverse of the previous method. In the case of the dorsal dislocations we circumduct outwards; in the reduction of the two forward dislocations we circumduct inwards. The patient is put on his back in exactly the same way, the limb is flexed, and then it is circumducted inwards. That rotation tends to bring the head of the bone downwards towards the acetabulum, and with a little traction it slips into its place.

The Y-ligament is equally important in both kinds, and it is very important that you should not tear that. I have known such force used that the ligament was torn through, in which case you get that curious condition in which you can put the limb in any position you please. It is an easy experiment to make on the dead body in a thin subject.

So much, gentlemen, for this case. I think I have given you the principal points about it; and it just illustrates the importance of making a diagnosis immediately after an accident, and that, even if a mistake is made at the time, it is possible to repair the error later on. This man I have no doubt will get a perfect hip, his capsule will be quite sufficient to keep the bone in position; the ligamentum teres will not unite in all probability, and should we a great many years hence have the opportunity of examining the interior of the joint, it will be found that the parts are smoothed off there, but that no repair has taken place in the interior of the joint.

ON

A CASE OF PYÆMIA.

Gentlemen,—To-day I have to speak to you about an interesting case of pyæmia, which unfortunately proved fatal. It is particularly interesting because you so seldom see cases of pyæmia nowadays. In my student days, I am sorry to say, pyæmia was the curse of the hospital, most of the large amputation cases dying of pyæmia a week or ten days after the operation. But now, thanks to antiseptics, and to the greater care which is taken of the patients generally, cases of pyæmia are extremely rare, and, if I had a case of pyæmia originating in my wards, I should be inclined to inquire of my house-surgeon as to why the case had gone wrong. I beg you to remark that this patient was admitted into the hospital as a case of pyæmia.

Before I go into the case I should like to tell you what pyæmia means, and I will read you what Burdon Sanderson says in his lectures on the "Infective Processes of Disease" (*British Medical*

Journal, December 29th, 1877):—"What I mean by septicæmia is a constitutional disorder of limited duration, produced by the entrance into the blood-stream of a certain quantity of septic material. It must, therefore, be regarded not so much as a disease, but as a complication, differing from pyæmia not only in the fact that it has no necessary connection with any local process, either primary or secondary, but also in the important particular that it has no development. Pyæmia is a malignant process which goes on and on to its fatal end; but in the case of septicæmia, inasmuch as the poison which produces it has no tendency to multiply in the organism, there is no reason why the morbid process should not come to an end of itself, unless either the original dose is fatal, or a second infection takes place from the same or another source. The one is septic intoxication, due to the absorption of chemical products of decomposition, exceptionally attended by local mischief; the other embraces a group of diseases of which one, at least, is due to the multiplication in the system of a pathogenic organism, and in all of which metastatic inflammations so frequently occur that their presence is held to stamp the disorder as pyæmic, even when no evidence of a germ can be found."

The case which we have to consider to-day was one of pure pyæmia. A married woman, 24 years of age, was admitted to the hospital on December 28th, 1894. On November 18th the patient was confined of a full-term child after a difficult labour, with instruments. She went on well until about the end of November, when she noticed that her left knee was slightly swollen. The swelling increased, and the knee became very painful. Her doctor painted it with iodine and applied mercurial ointment, but as the knee continued to get larger he advised her to come to the hospital.

A week before the confinement the patient had been in bed with what was said to be influenza. The temperature went up shortly after the confinement, and on the sixth or seventh day it was decided to clear out the uterus, which was done on the ninth day by curetting it and syringing with iodine lotion. The discharge was not offensive, but a small piece of placenta was removed. The next day the temperature stood at 103° ; on December 1st it fell to 99.8° ; the next morning it rose to 103.8°, and it was on the 1st of December that the pain appeared in the knee. Between the 2nd and 18th of December the temperature varied, was normal on three occasions, and generally ranged between 100° and 102°. The first child was born two or three years ago, when there were no instruments used, the delivery being easy. The patient had a miscarriage at two or three months in November 1893, and had a second child in November 1894, from which illness the present case dates.

When she was admitted to the hospital, we found a young woman, well developed, but rather pallid and of a yellowish tint; she was in bed, and one knee was enormously swollen. There could be no question that the knee contained fluid, and from the history of the case I was quite certain that it must be pus. Accordingly, we gave the patient an anæsthetic, and made a free incision on each side of the affected knee, and let out twenty-seven ounces of pus. Large drainage-tubes were inserted, and pushed well up into the pouches of the joint. The joint was syringed out with a weak carbolic lotion, and dressed with tarred oakum pads. So far as the knee went, nothing could have done better. It was syringed out twice a day, and the tubes were kept in for some time, and then removed. Really, so far as we could see, everything went well with regard to the knee. The wounds practically healed, and when the patient died the continuity of the joint had been quite restored. But the patient never rallied as I hoped she would have done. The best proof is the temperature table. You will see that the temperature at the operation was 101°, then it dropped, then oscillated for the next day or two, after which it rose to 102°. It again worked up and down, and on the 3rd of January it reached

103.5°. Against that, in my own handwriting, is the note "Christmas-tree." This was because I thought and hoped that the excitement and bustle in the ward due to the Christmas-tree festivities might have sent her temperature up. But the temperature did not come down; as you see, it kept up to between 102° and 103°. It did once drop just below the normal, but on the whole it has maintained the characteristic rise and fall of a blood disease. And here, at the end, when the patient was much weakened, we find at times the temperature falling below normal. She at last died on February 4th, having been in the hospital thirty-nine days.

You have all seen the patient, and I hope you have studied her appearance, because it was very characteristic. Now, cases of this kind are almost hopeless from the first—that is to say, if they are *bonâ fide* cases of pyæmia. I have seen cases of pyæmia recover, but the majority are almost certain to die. But that does not absolve us from the necessity of trying to save them if possible, and the way to save them is to evacuate the pus, and to take the strictest precautions to keep the patient's wound aseptic, and then to adopt a stimulant and tonic plan of treatment. The first time I saw the patient, after opening the knee I ordered her to have 5-grain doses of quinine, which she was to take every four hours, and I put her on six ounces of brandy at once. In giving brandy you should not give it by tablespoonfuls, but by teaspoonfuls, since it is better for a patient suffering from blood-poisoning that the necessary stimulant should be taken in small but repeated doses. This patient had two teaspoonfuls of brandy every hour or two hours, according to circumstances. A teaspoonful of brandy every hour for twenty-four hours equals three ounces *per diem*, so that two every hour make six ounces. We had afterwards to increase the amount to nine ounces in the twenty-four hours, but we did not exceed that. When I was a student, the late Dr. Todd was promulgating the doctrine that stimulant treatment was *the* treatment for all acute diseases and in some ways, no doubt, he carried his views to excess; but, at any rate, he taught surgeons that brandy or some form of stimulant is *the* medicine in cases of blood-poisoning. I am quite sure of this, that I have seen cases which have been saved by stimulant treatment, particularly cases of septicæmia, and occasionally a case of pyæmia. In the present case we did all that could be done, getting her to take all the nourishment she could, and giving her stout in addition to help the food down.

We were constantly on the look-out for secondary abscesses. I had her lungs carefully examined; I examined her abdomen, and found she had a large liver. But it was not till we were in the post-mortem room that we became aware that there was a secondary abscess in the region of the shoulder. Now that abscess, I am quite sure, must have formed within forty-eight hours of her death. If it had been formed before, I am certain it would have been noticed by the nurses or the sister. The patient was in a state of coma for sixty hours before death, and it is nearly certain that the abscess formed within the last two days of the patient's life. We opened the abscess in the post-mortem room, and found that it contained about six ounces of thick green pus, of peculiar odour. The abscess was lying beneath the deltoid, and also beneath the periosteum of the humerus, the bone around being necrosed. The shoulder-joint was not diseased, as the abscess was outside it. As to the knee, we examined it, and found, on turning back the skin, that there was a small collection of matter on the inner side, below the joint which had been laid open. This abscess was in the tissues which had not been interfered with by the two incisions at the operation. When we came to open the knee-joint we found it as healthy as could be. My fingers had been in the joint, my house-surgeon had his fingers in the joint, and we had drainage tubes in it, yet, notwithstanding all that, there was not the slightest change in the synovial membrane or in the cartilages—in fact, there was really nothing the matter with the joint.

We went on to see if there was any trouble elsewhere. The liver weighed 6 lbs. 4 ozs., and was soft, large, and pale; it did not present the appearance of a hob-nail liver, and yet I cannot help thinking it was connected, in some sort of way, with drink. Taking into consideration the state of the liver and kidneys, I am of opinion that the woman must have indulged in alcohol. We then found that the spleen was enlarged, and slightly softer than normal. The pericardium contained very little fluid, and the heart was normal. The right lung was also normal, while the left showed a small area of congestion near the base at the back. The pleuræ were healthy.

Now we come to the important point as to what was found in the pelvis. The uterus measured $2\frac{3}{4} \times 1\frac{3}{4}$ inches, and, except for a few peritoneal web-like adhesions attached to the posterior surface, appeared to be quite healthy externally. On laying the uterus open, the placenta seemed to have been attached to the upper part of the posterior wall, and the placental site appeared to be healthy. On section, the uterine wall appeared to be normal for the period after delivery. There were no thrombi to be seen in the venous sinuses of the uterine wall. The Fallopian tubes were normal. The left ovary contained a *corpus luteum*,

$\frac{1}{4}$ inch across. The ovaries were healthy. There was no lesion of the vaginal mucous membrane, but the cervix had been torn bi-laterally. I think that is most important. It seems to me that the mischief must have spread from that, rather than from the patch of retained placenta. I should imagine that little bits of placenta come away without much harm resulting sometimes, and it really appears as if the retained portion of the placenta was not the source of the evil, as the sinuses of the uterus were perfectly healthy. The left uterine veins were thrombosed in the broad ligament, and contained broken-down clot. A similar clot was present in the internal iliac, the common iliac, external iliac, and femoral veins, as far as the middle of the thigh. Their contents were more purulent than those of the uterine veins. There was slight thickening of the tissues in the pelvic wall, round the left internal iliac vein.

Although, therefore, this patient survived her confinement from November 18th till February 4th, the mischief was entirely connected with the delivery, and she died from thrombosis of the veins and from pyæmia—the result of the breaking down of these clots. In this preparation from the museum you see veins from a leg with thrombosis and phlebitis. The patient died from erysipelas and pyæmia after amputation of the great toe. Remember that the mere formation of thrombi in a vein is by no means a very serious thing. We are constantly having patients with thrombosis of the saphena vein, an ordinary case of varicose veins in which thrombosis may occur accidentally. These get perfectly well, because the clot is not septic, but a septic clot is sure to break down, and a condition of pyæmia is then produced.

Of course the word "pyæmia" is a misnomer. It was formerly thought that pus cells could be seen in the blood. The early microscopists mistook the ordinary white corpuscles of the blood for pus cells, and it was said that pyæmia could be detected by seeing the cells floating in the blood. We know now that pus does not circulate in the blood; but that the tendency is for pus to be secreted, owing to the poisoning of the blood circulating through the body. Especially is this tendency most marked where there is great vascularity. You therefore expect this secondary deposit in a large joint, and the knee-joint is one of the largest. Next to the knee-joint it may be found most commonly in the shoulder, and very commonly we find secondary abscesses in the liver; but I had this liver very carefully searched, and could find nothing of the sort. So also in the spleen, in the lungs, and in the kidneys these abscesses sometimes occur, but there was no evidence of that.

I have been speaking till now of fatal cases of pyæmia; but a year ago we had in this hospital a case somewhat similar, which, however, was not fatal. The patient was a young woman of 22, married. She had never had any illness before, and had a child on December 27th, 1893. Four days afterwards she noticed that her hand and arm were swollen and very painful. This gradually grew worse till the following week, when an opening was made in her wrist by her doctor—I think it was her left wrist—a large quantity of pus and blood was drawn off, and a fomentation applied. The swelling in the arm subsided, and the patient felt a good deal easier. On the 13th January, 1894, the patient began to feel some pain in the right hip, which prevented her walking; it was swollen and very tender. She had rigors soon after she had noticed the swelling of the arm, and had these attacks two or three days before the incisions were made in the wrist, and they lasted the entire day. She was admitted here on January 27th, 1894. This happened to be a Wednesday, and I did not see her until the following Friday, the 29th. When I saw her, the condition was that of a fairly well nourished woman of 22, with no excess of subcutaneous fat, the face was rather pale, mucous membranes also pale. On admission her temperature was 103°, which fell early on the 28th January to 100°. It rose in the evening, however, to 102·4°, and on the morning of the 29th of January was 100·4°.

There was no tenderness on deep pressure above the pubes, the uterus could not be felt through the abdomen, and on vaginal examination nothing could be felt abnormal ; the breasts were typical of lactation, and not painful. The left wrist was swollen and tender, and there were two sore places covered with rather pale granulations, one at the base of the first metacarpal, and another an inch in front of it. The anterior sore was an inch and a half across ; the posterior sore seemed to lead into a sinus, from which turbid fluid escaped on moving the hand. A probe passed the distance of one inch inwards, and touched on bare carious bone. The right hip was very painful and tender, and was most comfortable when extended and inverted. It could be flexed, but movement of the joint was painful, though not severely so, and the joint was by no means fixed. Jarring the head of the femur against the acetabulum did not cause pain, and there was no grating in the joint. The front of the joint was slightly tender, but was not swollen. The gap between the ischial tuberosity and the great trochanter was filled up by an extremely tender swelling, in which fluctuation could be obtained.

Here was a case with a history of a confinement, with an abscess in the left wrist, and an abscess in the right hip, and when I saw the patient her condition was very serious. Her temperature was then up to 103°, and altogether she was exceedingly ill. It became a nice question—Was that matter in connection with her hip-joint or not? I concluded that it was not in direct communication with the joint, but was outside it. In arriving at this conclusion I, in the first place, went by the movements of the hip, and in the second place by the inversion of the thigh. If the joint was full of matter, I should have expected to find it everted; but, on the contrary, it was inverted, and there was also the soft swelling between the ischial tuberosity and the great trochanter. So I had the patient brought down into the theatre, an anæsthetic was given, and I punctured with a large aspirator needle and found that I got matter. Thereupon I made a free incision and introduced my finger, and although the matter was under the glutei muscles the joint itself was not involved, and I carefully abstained from opening into it. Then I turned my attention to the wrist. I found the scaphoid bone was loose and carious, and I therefore took it out, but the other carpal bones were left. I had the hand put up on a back splint with the fingers extended, and I hoped the result would be a useful hand. As regards the buttock, after gently scraping the cavity with a sharp spoon, and thoroughly syringing out with carbolic lotion, a large drainage tube was put in. We then pursued the same treatment internally as in the other case—five grains of quinine, with thirty minims of dilute hydrobromic acid, in an ounce of water. This was at first ordered three times a day, and then I find I altered it myself to every eight hours. I did that because when you give quinine in large doses you want the doses distributed over the whole twenty-four hours, and that is an important point. I also gave her four ounces of brandy *per diem.* That woman recovered; she got perfectly well, and went out. The abscess at the hip closed and filled up, and the abscess at the wrist closed also. Although she had some little deformity and slight stiffness, she went out perfectly able to walk and in very good health, after being in the hospital fifty-two days. So you see it is possible for a patient to recover, even after pyæmia, though of course a good deal depends on the previous condition of the individual.

Let me give here, in illustration, another little extract from the article on pyæmia:—"It is even probable, as showing the influence of these predisposing causes, that the tissues of the human body, so long as they are healthy, possess the power of resisting the action of these germs—perhaps even of destroying them. Burdon Sanderson has shown, with regard to bacteria producing the poison of septic intoxication, that even if they do enter they have no effect on the healthy organism; and this principle may have a much wider range of application, serving, for example, to explain the susceptibility to scarlatina of patients suffering from traumatic fever." Let me here say that neither of these women was in perfect health—they had both gone through the perils of childbirth, and were therefore predisposed to the attack of an unhealthy action. We all know, or ought to know, that puerperal women are especially liable to an attack of any disease which is near them, and it is for that reason we exercise such great care. No man is justified in going to a puerperal woman's bedside after having seen recently any infectious case; the disease would probably be taken, and the patient would be sure to do badly.

Let me say one word with regard to these cases, whether they be of septicæmia or pyæmia, you cannot go wrong if you use vigorously supporting and stimulating treatment. The younger generation of medical men does not appear to be always clear on this point. Some years ago I was called to see a gentleman who had clearly got pyæmia or septicæmia, and I was inclined to believe it septicæmia. When I met the doctor I asked what the case was. He said, "a bad case of septicæmia, and I am afraid he will die." I saw the patient, and I also was afraid he would die. I asked the doctor what he had been doing for him, to which he replied: "I am giving him one grain of carbolic acid three times a day." I did not say anything, it would have been of no use; the medical man evidently had not grasped the situation. I only said it was very good treatment so far as it went, and added, "If you will get home as soon as you can and send a messenger over with some five-grain powders of quinine, I will, in the meantime, get up some champagne from the cellar and see what we can do." Now, here was a rich man, dangerously ill, with any amount of stimulant ready to hand, and yet he had never been offered any. Well, I got up the champagne and gave some to my patient, at the same time impressing upon his wife to go on with the champagne and to add brandy to it, giving him soup and everything nourishing. But the end of it was the poor fellow died. I did what I could to cover the retreat, but it was not of much use, for the friends of the patient, naturally enough, said, "It is all very well, you may say what you like; the doctor may have done all that he could according to his knowledge and power, yet it is perfectly obvious that Mr. Heath took a different view, as any one can see from his change in the treatment." What is it possible for any one to say in a case like that? Fortunately the thing blew over, but the fact remains that the patient's life was probably lost from the case not having been taken in time.

In the case of the woman with the bad knee, the gentleman who attended her had obviously not recognised what was the matter with the knee. You can see this from his having used iodine and Scott's dressing. I do not like the use of iodine myself, and, as for Scott's dressing, it is a remedy useful only in its proper place; but where you have an acute case, with fluid in the joint, the probability is that there is pus, and there can be no question about the treatment. You will have noticed that in the second case I used an aspirator needle, though I was sure there was matter there. I should strongly advise you, before you lay open a joint, to use an aspirator needle, to make quite sure it is matter before making a free incision. In aspirating use a large needle. I was once myself unintentionally misled by a practitioner over a case of perityphlitis. He assured me that he had aspirated and found pus, but when I made an incision there was none. The mistake arose from the practitioner using a hypodermic needle. This had been carefully cleansed with carbolic lotion, and serous fluid passing through a small needle recently cleansed with carbolic gives an appearance very like pus, and probably led to the mistake. The proper treatment is to open freely every abscess in a case of pyæmia as soon as it is formed. I regret we did not know of the abscess in this poor woman's shoulder, but there is no doubt it formed very late in her life, and the omission made no difference. The only wonder is that she lived as long as she did. More could not have been done for her, as the case was hopeless from the beginning.

It is worth while to always make a good fight, however, because every now and then you will succeed in pulling a patient through, although I am sorry to say our efforts in this case were of no avail.

ON

LATERAL CURVATURE OF THE SPINE.

GENTLEMEN,—We have in the ward a young woman with lateral curvature of the spine, a malady which comes under notice far more frequently in private than in hospital practice. We are often asked by anxious mothers if there is anything wrong with their children's backs, and this anxiety is not unnatural. Few backs are quite straight, and in many young people there is a strong tendency for the trunk to lose its habitual equilibrium, and for the spine to lose its perfect straightness. We meet with two classes of lateral curvature, according as the curvature begins in the the dorsal or in the lumbar region. A common instance of the first class is in a girl, say of 16; the dressmaker says: "Your shoulder is growing out," and in most cases it is the right blade-bone which is more prominent than the left. The mother is alarmed, and at once takes her daughter to her doctor for examination.

Now, you can examine a girl's spine without any unpleasant exposure, if the clothes are properly pinned up round the pelvis. I show you here a diagram of the back of a healthy woman. Note that under normal conditions the scapulæ above, and the folds of the buttocks below, are perfectly level. But if one foot is placed upon a book the crest of the ilium is raised, the pelvis is sloping, and a curve is produced in the lumbar spine. This fact is important, because in many of these cases the pelvis is not straight, one leg being a little longer than the other. Few persons have eyes of precisely the same focus, and few have legs of quite the same length. A trifling illness in early life, with pain in one leg, perhaps in the hip, may have checked the growth of that limb. The effect of even a slight difference in length is to cause a tilt of the pelvis, the lower side corresponding to the shorter leg.

The tilt of the pelvis accounts for the lumbar curve, but it only accounts for the dorsal curve when that is secondary to the lumbar curve, and is in the opposite direction, to preserve the equilibrium of the trunk.

There is another class of cases, however, in which the dorsal curve comes first. A curve may begin there in consequence of a severe attack of pleurisy or from an empyema, which leads to a contraction of one side of the thorax; but quite apart from this we have curvature beginning in the dorsal spine, and I believe the correct explanation is that which has been given by Mr. Barwell. The serratus magnus, which determines the fixation of the scapula in the movements of the arm, tends to pull the ribs into an abnormal position if more work is thrown on one side than on the other. The connection of the ribs with the vertebræ causes the action of the serratus on the ribs to produce a rotation of the vertebræ on their vertical axes. The altered position of the ribs involves an undue prominence of the scapula on that side, and then the girl's shoulder is said to grow out. But other causes are often in operation.

Habitual muscular action is often influential, and many young girls have more work put upon them than they can stand. Among the poor they are set to carry children almost as big as themselves, and in the middle and upper classes they are made to practise the piano for hours, or to draw or to write with arms and body in a harmful position. Especially in writing at a flat table the elbow is fixed, and there is a strain on the scapula which the serratus magnus opposes by pulling on the ribs, and the effect is to cause a gradual rotation of the vertebræ. This result is slowly produced, but habitual posture in the course of months has a powerful effect. The ribs are drawn backwards and the scapula stands out. This theory of the influence of the serratus magnus may not be all the truth, but after thinking the the matter well over I am convinced that it is true, and it is especially confirmed by the place at which this dorsal curve begins.

On looking at the patient before you, you see how the scapula projects, and you can notice that it is due to the backward prominence of the ribs under it. When the patient folds her arms and stoops forwards you see the curve in the dorsal spine. We can only perceive it by the spinous processes; the transverse processes are concealed beneath the muscles, or they would show us the rotation of the vertebræ on which the curvature depends. There is in this case a history of a chest illness in early childhood, which was probably an attack of pleurisy, determining a change in the position of the ribs, until you now see the ultimate consequence.

In another patient I show you there is such a curve in the lumbar spine as to bring the last rib on one side almost to the edge of the pelvis, so that I can hardly get my finger in between them. The pelvis is not level, and if I mark with chalk the position of the spine you will see the curve. I put a book under the foot of the patient on the lower side, and the deformity partly disappears.

It is important to measure the legs in these cases, but a little tact is wise, and it can be done effectually without unpleasant exposure. If you try to measure the legs with a tape you are very likely to be misled, but make the girl lie flat on the floor and draw up her legs by bending her knees. If one knee is lower than the other, the feet being even, that leg is shorter than the other. But a still better test is to observe the pelvis and lower part of the back when the patient is standing and the feet together.

I am sorry to say that lateral curvature of the spine has been the cause of a system of practice which is a disgrace to our profession. I show you some of the apparatus such as I have again and again taken off the backs of girls whose spines were perfectly straight.

I remember two sisters being brought to me fitted with new instruments. I had the supports taken off and examined one girl's back, and said: "She has certainly a slight curvature. I do not prefer the instrument she has, but it is a well-recognised method." And then I examined the second girl's back, and found that her back was perfectly straight; there was nothing the matter with it. "Well," I said to her, "your back is quite straight." "Oh, yes," she said, "I know it is, but the doctor said if I did not wear the apparatus it would soon be like my sister's!"

These spinal supports are made with the idea that, when you lift the girl up and hold her up with your hands, you straighten the back, and these machines are intended to do this; but you must remember that you are standing away from the girl, and it is another thing when you put an instrument upon the actual patient herself, and expect that to do what you have done from quite a separate base. Then you see cog-wheels to increase the pressure on the ribs, and there are crutches to support the arms. The gentleman who orders the instrument keeps the key. The mother and daughter have to go to him once a week in order that the instrument may be screwed up and adjusted, but a few turns reach the end of the adjustment, and then it is necessary for him to unscrew it and begin afresh. It is a monstrous thing, and, as far as I have seen, no good results from it, or can result from it, except to the pocket of the gentleman who ordered it and the instrument-maker who made it. There are a few, a very few, cases in which a support of this kind, with crutches under the arms, may be of use, but as a rule it is harmful.

I do not say that I never order apparatus, but when there is a primary curve in the lumbar region all that is really necessary is to rectify the shortness of one leg, which is the common cause, by a compensating boot. But I do not compensate by adding to the boot for the shorter leg. If, for instance, the left is half an inch shorter than the other, the bootmaker should take a corresponding amount from the heel of the right boot. Remember that to get such a thing done properly you must take the trouble to write down the precise instructions as to what is to be done. The object is to get the pelvis straight by raising one leg or by lowering the other. It is better to first lessen the thickness of the sole of the boot on the longer leg, and only to add to that of the other if the first is insufficient. If an apparatus is needed it should be worn only in the daytime, and be carefully attended to so that it does not make the skin sore by chafing.

Some apparatuses are simply stays with supports added to them; but the important thing that should first be attended to in such cases is to see that a girl has plenty of room in her clothes. Make a girl unbutton her dress and fill her chest by a deep inspiration. You will then often find two inches between the buttons and the button holes. When the dress is fastened it is impossible for her to fill her chest with air, and the first thing is to get rid of this hindrance to expansion. The best method to adopt with such cases, in order to secure the filling of the chest with air, is to have a rope put up in some convenient place, with knots on it. Let the girl, with as little on as may be, try to climb up the rope until she is on tiptoe, then take three deep breaths and let herself down again gradually, and presently repeat the proceeding three times. I am accustomed to write the directions down on paper, and make her keep it in her bedroom. It is well to write these things down with every formality, as if it were a prescription. The patients may not carry out your prescription, but they are more likely to do so if you go through the formality.

Next, it is most important that the patient should avoid any habit tending to throw work on one shoulder more than on the other. Too much work should not be thrown upon the muscles of the back. In piano playing, for instance, it is well that the back should have a firm support behind it, and when children play the piano take care that the feet are always upon a stool and not dangling down. Take care also that all writing and drawing is done at a sloping desk, the girl sitting on a chair that is not too high.

With regard to exercise in these cases, if the patient can ride a horse or a pony it is a good thing, but a girl should not always ride with the crutch of the saddle on the same side. Small side saddles are made for children with a crutch that can be shifted.

Shall the girl be made to lie down or not? I believe that lying down is not of the slightest use in these cases. The practice is an old one, and it unquestionably rests the muscles, and it is wise to make a girl lie down for an hour after a ride or walk, but simply to rest the muscles, and not with the view of acting on the spine.

Such is the general treatment of these cases. As far as you can, avoid special stays. There are some severe cases which require instrumental supports, especially cases in which bony deformity has resulted from rickets; but you should assure the parents that they are not to cure the disfigurement, but simply to prevent it becoming worse.

Some years ago I served on a committee appointed by the Clinical Society to consider the question of the treatment of lateral curvature of the spine, and I will read to you some of the conclusions we came to.*

"*Cases without Osseous Deformity.*—The Committee feel that while gymnastic exercises prove very useful in a great many cases of pronounced lateral curvature by improving the muscular power and relieving pain, yet that it is essentially in cases without osseous deformity, and when the curvature depends on flaccid muscles, relaxed ligaments, and on a general want of tone of the system, that this plan is indicated, and most likely to prove beneficial and even curative. Moreover, it seems not improbable that the use of mechanical supports in such cases tends to maintain the curvature by preventing or limiting voluntary muscular action, and thus withdrawing from the muscles their natural stimulus to action and healthy tone. On the other hand, there are cases of lateral curvature in children too young to appreciate the dangers of faulty positions, and too weakly to keep themselves in improved positions after drilling without some extraneous aid. In such cases some means additional to exercises may be necessary to prevent the assumption of these faulty positions.

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* *Clin. Soc. Trans.*, xxi.

"*Cases with Osseous Deformity.*—For cases in which there is more or less well-marked osseous deformity treatment must necessarily vary according to the special needs of the case, a cure of the deformity, or even much alleviation, being quite impossible. In many of these cases the patient complains of pain, which pain is relieved by the use of a well-adjusted support, a fact to which the Committee can testify from personal observation. On the other hand, the Committee have seen great relief to pain afforded by gymnastic exercises without the use of mechanical supports. It seems very doubtful whether any real or lasting improvement is procurable, by any method of treatment whatever, in the later stages of such cases."

ON

STONE IN THE BLADDER.

GENTLEMEN,—On Wednesday you saw me remove these three stones from the bladder of a patient aged 45. He was admitted into the hospital from the Out-patient Department, where he presented himself complaining of pain about the bladder and trouble generally of the urinary organs; and from the symptoms he gave Mr. Drew thought it right to examine his bladder, and, on sounding, immediately detected the presence of a stone, and sent him into the ward. I proceeded to examine his bladder with a sound; and I then, without difficulty, detected a good-sized stone. Under these conditions, and, taking into consideration the fact that his urine was only slightly acid, and that it was very cloudy, with a good deal of bladder irritation, I decided on performing a supra-pubic lithotomy rather than lithotrity. That operation I did on May 8th quite successfully; and you have just seen the man—five days later—lying in bed perfectly well and practically cured. All I have done to-day is to remove the tube which was introduced at the time of the operation.

With regard to the symptoms of stone, you must remember that we constantly have patients coming here who think they have stone and who have not. You will recollect that only a week ago I sounded a man who was sent here under the impression that he was suffering from stone in the bladder, and you must remember that every one, I do not care who it is, occasionally makes a mistake in overlooking a stone. It is just as well, however, not to be too cocksure about the business, and decide as to the absence of a stone on one examination only; and I therefore told that patient to come again, that I might examine him under chloroform, so as to make quite certain.

Now the symptoms of stone are these: You find in the first place that the patient has increased frequency of micturition, and further that the frequency is more noticeable by day than during the night, which is just the reverse of what occurs in a patient with an enlarged prostate. A prostatic case is always worse at night, and gets completely worn out by his rest being broken through constantly getting up during the night; while on the other hand a stone case, once in bed and at rest, gets quite comfortable and his pain leaves him, but if he has to be about in the daytime, and especially if he has to go through any violent exertion, the pain naturally comes on severely.

A man suffering from stone in the bladder becomes very artful. He will tell you that he can ride in a tram without any inconvenience, but that he cannot manage an omnibus: obviously the tram being smoother in its motion makes all the difference. He knows, too, from experience that he has more pain when he empties his bladder, the reason being, of course, that the empty bladder comes in contact with the calculus, and thus pain is produced, which is referred very generally to the end of the penis. In children that is very marked, and you see that a child will pull at its penis, and sometimes elongate the foreskin very considerably, from the irritation arising from a stone; and you will find every now and then that a child is circumcised under the impression that it is suffering from an elongated foreskin, while it really has stone in the bladder.

After that the next most important symptom is the occasional passage of blood. The patient passes every now and then a little blood, and more particularly after any exertion. Your patient will recognise this relation of blood to exertion, and, if a hunting man, will say that he notices if he has had a hard run that the bleeding is worse than when he has had a quiet day.

If you have, then, these symptoms—increased frequency of micturition, pain after emptying the bladder, pain at the end of the penis, and occasional passage of blood—you will conclude that the patient probably has a stone, and you will be quite justified in sounding that patient; and you will have no difficulty in touching the stone if it is anything of a size.

Now, you may remember a case in No. 1 Ward sent up to me last winter by a medical man, who said he thought the patient had stone in the bladder. I sounded him in the ward and could not find any stone; still, following the rule to sound a second time under chloroform, I had him brought into the theatre, and sounded him with a lithotrite, and found, rather to my surprise, that the lithotrite had caught a small stone, which I at once crushed and extracted. It was so small that I had failed to detect it with a sound; but when the stone is of any size, such as these three, you can have but little difficulty unless there is an enlarged prostate. And then there comes in this fallacy: The surgeon, in the case of an enlarged prostate, may not push the sound completely into the bladder. He uses the sound in the usual fashion, rotating it in the prostatic urethra, and may easily fancy that there is no stone present; but if he pushed the sound well up into the bladder he would find the stone. You should make sure, therefore, that you have pushed the sound well up into the bladder, and, directly the sound has entered, turn it down and explore behind the prostate. Then comes the question, Shall you or shall you not have some water in the bladder? Well, you ought to have some, and I take it that whatever is in the bladder at the time will probably be enough, but if you fail to detect the stone there may be more water than you want, so it is convenient to use a hollow sound to enable you to draw the water off, and as you do so you may feel the stone click against the sound. It is possible, of course, to make a mistake. It has happened that growths in the bladder, which are coated with phosphates, have been mistaken for stone. It is said, too, that in the case of children mistakes may arise through the sound striking the pelvis. In this particular case there was no question at all. Here was a man of 45 years of age with a large stone in his bladder, and I therefore resolved to do the supra-pubic operation.

THE SUPRA-PUBIC OPERATION.

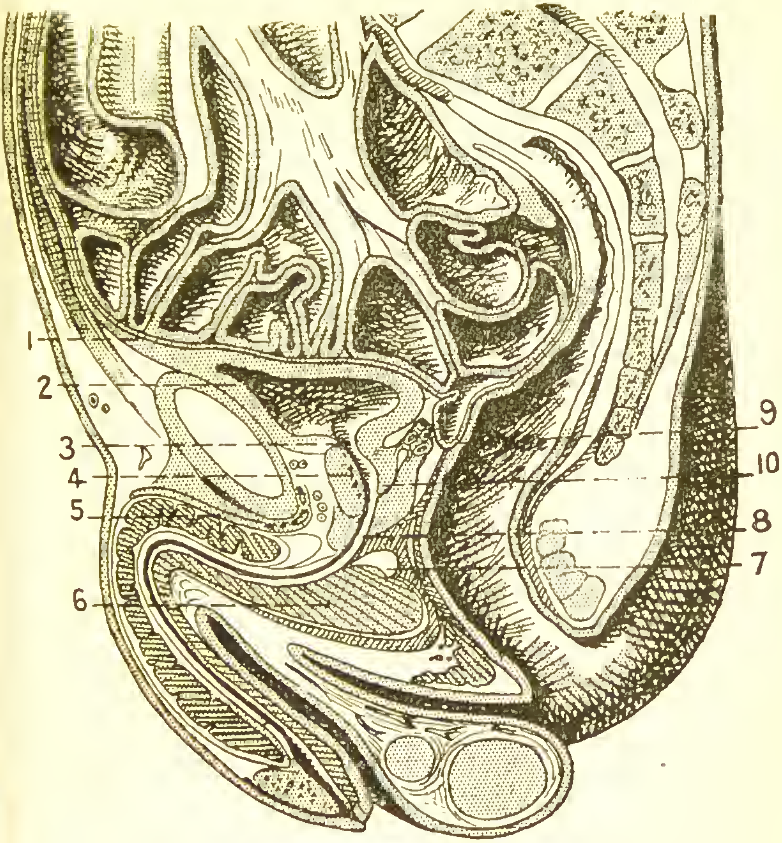
This operation is a revival of a very old proceeding, an operation which was done, so far as we can tell, about the year 1560 by a French surgeon, Pierre Franco, and it appears to have arisen in this way: He was going to remove a stone by the perineal operation, but found that it was so large that he could not do so; and then he found that the stone could be easily pushed up above the pubes, and accordingly he cut down upon it there and took it out without difficulty, and the patient did perfectly well.

Cheselden, who was a great lithotomist in this country about the year 1700, and whose bust you may see at St. Thomas's Hospital, and whose portrait, when you are fortunate enough to get your diploma, you will see in the Council Room at the College of Surgeons, took up the operation, and then seems to have abandoned it in favour of the lateral perineal operation. Although the operation of supra-pubic lithotomy was occasionally performed after its first introduction, for the last 250 years the operation has practically been disused. What brought it into notice again was this: that a British anatomist, Dr. Garson, drew attention in the *Edinburgh Medical Journal* for October 1878, to the effect on the bladder of distending the rectum.

Here is Dr. Garson's diagram of a frozen section of the bladder and rectum in their natural positions, and in this other diagram is shown the effect of the distension of both viscera: the front of the bladder is in contact with the anterior wall of the abdomen, and you will notice that the space between the peritoneum and the top of the pubes is about 1½ inch, which would be quite sufficient to allow of a supra-pubic lithotomy (Figs. 24, 25).

This paper did not attract much attention among surgeons, but, two years afterwards, in 1880, Petersen, a Danish surgeon, of Kiel, quite independently promulgated the notion that if you distended the rectum, and at the same time the bladder, you would get the effect which Dr. Garson had already pointed out. So Petersen invented the rectal bag, which is a sausage-shaped or conical india-rubber ball, to be introduced into the rectum and distended with fluid, and the bladder, being also filled, is raised, and the peritoneum is thus pushed up out of the way. This plan gave a great impetus to the operation, and it has been very generally employed since in this country and in other countries, and with very great success. But you may have noticed that I did not use Petersen's bag, and that I did not distend the bladder with water, but that I preferred distension with air. It is the first time I have done it, and I think it is an extremely good plan. The suggestion was made by Bristowe of Brooklyn, to Dr. Keen of Philadelphia, who was, I believe, the first to employ air. You saw that I put a catheter into the urethra, and, having first washed out the bladder as a precautionary measure, I then emptied the bladder again and distended it with air, and purposely left the catheter still in so that I could

FIG. 24.

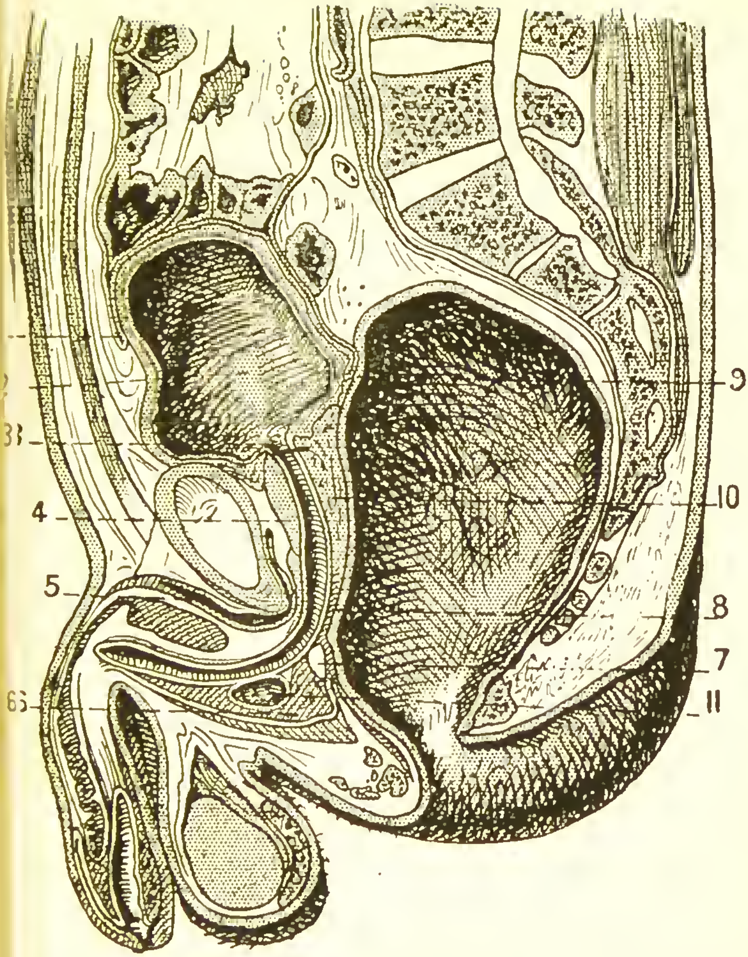


Longitudinal section through a frozen body with the rectum filled (naturally), but not distended. 1. Peritoneal fold passing over the bladder from the abdominal walls. 2. The bladder. 3. Internal orifice of urethra. 4. The prostate. 5. Dorsal vein of penis. 6. Bulbous portion of urethra. 7. Cowper's glands. 8. The upper end of the membranous portion of the urethra. 9. Peritoneal fold forming Douglas's pouch. 10. Prostate.

put more air in if necessary, and also that I might use the catheter to raise the apex of the bladder if I wanted to do so. Having felt that the bladder was fairly well distended, I made an incision immediately above and on to the pubes about 2½ inches in length. I then worked in carefully between the muscles, and having exposed the fat and cellular tissue I came on the bladder with my finger, and then, in order to keep it within reach, I passed two threads with a curved needle, one on each side, to hold the bladder in position: next I scraped a little on to the muscular fibres of the bladder, taking care to avoid some veins, and for this I used a little ivory instrument, which was a suggestion of Sir Henry Thompson's. I was then able to open the bladder in the middle line without any difficulty, and the air immediately escaped.

There is this advantage of the air over the water distension, that the air escapes easily and there is no mess. There is also another consideration: if there is a sudden rush of water it may wash out with it a small calculus, which may get lost. Now this actually occurred to an eminent surgeon, a friend of mine, who in the presence of a large class opened a bladder for stone and no stone could be found. My friend was quite sure that he had felt the stone but he could not find it, and he went away in great dudgeon; but the stone was found afterwards in the blanket of the

FIG. 25.



Longitudinal section through a frozen body with the rectum much distended (artificially). 1. Peritoneal fold passing over the bladder from the abdominal walls. 2. The bladder. 3. Internal orifice of urethra. 4. The prostate. 5. Dorsal vein of penis. 6. Bulbous portion of urethra. 7. Cowper's glands. 8. The upper end of the membranous portion of the urethra. 9. Peritoneal fold forming Douglas's pouch. 10. Prostate. 11. Abscess in the bulbous portion of urethra.

patient, having been washed out in the rush of water.

Here let me say that no one, however eminent he may be, is justified in cutting into a bladder for stone under any circumstances whatever without first sounding the patient at the time of the operation. It is one of the rules of surgery, which ought never to be forgotten, that you are bound to sound the patient on the table. It is not sufficient to have sounded the patient the day before; it must be done at the moment of the operation. It occurred once to Liston, here in this theatre, on sounding a child's bladder not to find a stone; he sent the child away to bed, and again sounded him the following week, and found a stone easily and removed it, and I give you that as an example of what ought to be done. I have known a surgeon cut into a bladder for stone, being contented with somebody else having some days before sounded the patient, and in that particular case there was no stone in the bladder.

In regard to the after-treatment I have but little to say. We put in a flanged india-rubber tube, and we fastened a sheet of oil-silk beneath the flange and attached it to the pubes with plaster. The man has been lying perfectly dry and comfortable in bed, the urine being soaked up by moss pads, and to-day we have removed the tube, and in another week or so the opening will be closed up. The patient was relieved from all pain, and has eaten and slept well from the first.

Then with regard to the mechanical part of the operation. In the first place, you may have noticed that I stood, as I always do, on the patient's right hand. It is much more convenient when you sound a patient to stand on the right; taking the sound in your left hand to pass it along the urethra, and then into your right hand to enter the bladder and sound. That is what I did with the silver catheter which I used as a sound; then, still standing on the right side, I made my incision from above downwards, and I found no difficulty in slipping over my finger a pair of slightly curved forceps, and picking out the three calculi one after the other. The largest weighs 247 grains, the next 93 grains, and the smallest 36 grains, or a total of 376 grains. Now it is not always quite so easy to pick stones out of a bladder, for you may have to deal with a very rough calculus, a mulberry stone, which may be partially adherent to the coats of the bladder, or again you might have a stone which has become encysted, that is to say, where the stone is in a pouch from which you have to dislodge it with a scoop. Or it may happen to you, as it did to me once, to have so large a stone to remove as to make it difficult to take it away. Here is the stone, which weighs nearly 11 ounces, and that stone I took out of the bladder of a patient in the year 1890—on a Sunday I remember, and I never had such a sultry afternoon's work in my life.

The remarkable thing about the case was that the patient had suffered from urinary symptoms all his life, and the medical man who called me in had brought him into the world. The patient had always been a weakly lad and troubled with his waterworks, and yet for over thirty years, with these symptoms continually present, the patient had never been sounded. The probability is that the stone was there very early in the patient's life—possibly at birth even, for such a thing no doubt does occur. Immediately after the operation the patient did fairly well; but he only lived for three weeks, gradually sinking, worn out with kidney disease.

So much then for the operation of supra-pubic lithotomy, which I must say is an exceedingly easy one if properly done; but it is possible to make mistakes in it as in any other operation. I have myself seen the peritoneum opened in this operation, and it is naturally a thing to avoid. It is well to remember that you should always endeavour to have the bladder in as healthy a state as possible, so that if the peritoneum is opened as little harm may follow as possible. In the case I mention the peritoneum was opened and the bowels were exposed, but it was carefully washed out and then stitched up, and no harm came; still it is a very great point not to open the peritoneum.

CHANGES IN THE METHOD OF TREATING STONE.

Now just a word with regard to the changes which have taken place of late years in the treatment of stone. You are most of you probably aware that this theatre was the place where the great Robert Liston did most of his operations from 1836 to 1847, when he died; and it was crowded week after week by students and practitioners anxious to see Liston operate. I suppose he was one of the greatest lithotomists that ever lived; he had a very large number of cases of lithotomy and enjoyed very great success. The operation he performed was the lateral operation, and being a very powerful man, he cut boldly into the bladder, and then with a large pair of forceps he grasped the stone and somewhat roughly drew it out. You must remember that the bulk of his operations were done before the days of anæsthetics, when rapidity was a very great point. The patient was held by force in the lithotomy position, and the surgeon was naturally anxious to be as rapid as possible and thus save prolonged suffering.

Let me say, *apropos*, that I think it is much to be regretted that Mr. Pepys, who wrote *Pepys's Diary*—and I presume you all are acquainted with that classic—was not keeping his diary at the time he was cut for stone, about the year 1660. He alludes to the fact indeed very often, but he did not begin his diary till two years after he was cut. It would have been extremely interesting if a man like Pepys, who could so accurately describe even the smallest detail of his domestic life, had been able to give us his account of what he felt during the operation of lithotomy.

After Liston's death, when anæsthetics came more into vogue, of course rapidity was not so much sought after; and Sir William Fergusson, who was perhaps the next greatest lithotomist, was more deliberate. He was equally successful, but was more deliberate, and he made freer incisions into the neck of the bladder. That was at King's College Hospital; and when I was a student there, week after week lithotomy was performed with a large amount of success, especially considering the dangers that the patients then incurred from septic complications.

After Liston's time, of course, we had here Sir John Erichsen, who was also an excellent lithotomist, and then Sir Henry Thompson, who, I suppose, is the greatest living authority on stone. He devoted his attention to lithotrity, and I have seen him do four and five cases in one afternoon with remarkable success. He so much preferred lithotrity that he seldom did lithotomy, except under special circumstances. All these lithotomy cases were perineal, lateral or median operations, and a supra-pubic operation was only performed under very exceptional circumstances. But practically nothing else has been done here since that operation was brought into vogue by Petersen, and I know that Sir Henry has had a large success with the supra-pubic method in private.

You can hardly go wrong in a supra-pubic operation as you can in the perineal operation, and there have, in former years, been in this city and in other large cities melancholy cases of mismanagement of perineal lithotomies. I have myself seen one or two tragedies of the kind. I was assistant surgeon at another hospital to a surgeon who came, so to speak, a "cropper" with his first lithotomy on a child. The result was that he never would do another case, to my very great advantage, because I had all the lithotomies, and did a large number of operations in both children and adults. They were all, of course, perineal lithotomies. I have done a fair number here too, but of late, following the lead of others, I have adopted the supra-pubic method when the stone is too large to remove comfortably with the lithotrite.

In doing a supra-pubic lithotomy you are entirely independent of an assistant, you only require ordinary help ; but if you cut into the bladder from the perineum you require a skilled assistant to hold the staff, and the result of want of proper skill on the part of the assistant may react on the principal, and not only may mischief be done to him, but a catastrophe may happen to the patient. On the other hand, a principal may attribute his mishap to an assistant unfairly, as happened once to myself.

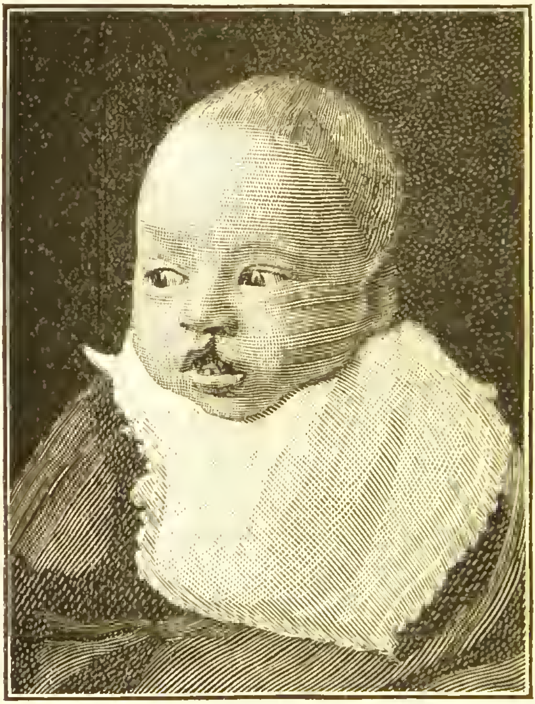
I do not want to trouble you with the details of perineal lithotomy, because you will probably never see it done. The supra-pubic method seems to have so thoroughly established itself among surgeons that one may reasonably look forward to its being the operation of the future.

ON

HARE-LIP.

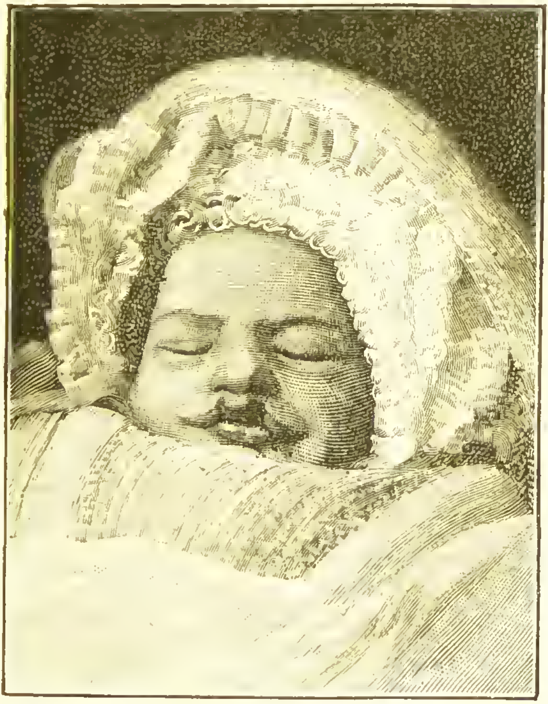
GENTLEMEN,—You have recently seen me operate on several cases of hare-lip, and it is important that you should understand the management of these cases, because it may happen to any one of you, soon after you get into practice, to deliver a woman of a hare-lipped child, and immediately the friends will want to know all about the case, the causes of the malformation, and the treatment that you propose to adopt. With regard to the malformation itself we have three principal varieties, viz., the single hare-lip, of which I show you a photograph (Fig. 26), and which is more usually, as in the photograph before us, on the left side of the lip. There seems to be no good explanation why this should be the case, but it is a well known clinical fact that single hare-lip is more common on the left than on the right side. Then we have the double hare-lip without any complication, of which I show you an example in a very fine child who had no other malformation whatever except a double fissure of the lip (Fig. 27). Lastly there come the very much more serious cases where we have a double hare-lip with a malformation of the alveolus and palate, and the photograph (Fig. 28), which I show you is a very good example of the kind. You will notice that at the tip of the child's nose there is a

FIG. 26.



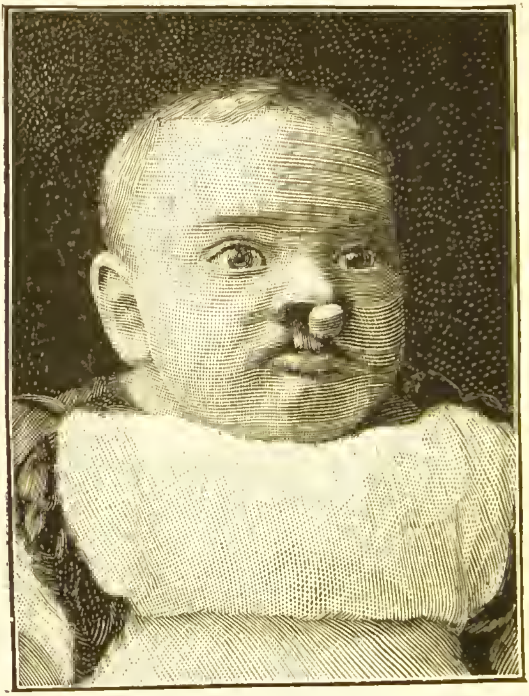
projecting piece which is really the inter-maxillary or pre-maxillary bone, and you will also see that the fissure is filled up to some extent by the child's tongue, which it has got into the habit of pushing forward. Now these last are by far the most serious cases. Of course it is a great distress to the parents that any child should be born hare-

FIG. 27.



lipped, but when we get a case of this very serious nature it becomes a very great question whether it would not be a relief to all parties if the child were to die. However, it is our business as medical men to save these children, and with a little care I think this can generally be done.

FIG. 28.



Now one word with regard to the cause of this malformation. It is no doubt an arrest of development. We are all hare-lipped at a very early period of our generation, but fortunately, as time goes on, we develop and our lips grow together, and in the great majority of instances there is no deformity. But if a woman in the early weeks of her pregnancy should get out of health, and particularly if her vital powers should be depressed by excessive sickness, which of course is common enough in the early weeks of pregnancy, then it may happen that an arrest of development takes place at a critical moment, and the result will be that the child is born hare-lipped. You will hardly find a mother of a hare-lipped child who is not able to give you some more or less rational account of some fright which she has sustained, and to which she attributes the malformation. It may be that she has actually seen a hare, and you will remember that in the hare the fissure is in the median line, but the more common story is that she has seen a hare-lipped individual; and sometimes there is no very definite account, but there is some idea that she has sustained a fright. Now in by far the great majority of cases, if you care to investigate closely, you will find that these shocks occurred long after the period to which we may attribute the formation of a hare-lip. They mostly occur in the later months of pregnancy, when it is too late really in any way to affect the fœtus.

Of course there is the hereditary element, and that cannot be ignored. There can be no question that a woman who is hare-lipped herself tends to have hare-lipped children, and we find that in some families there are a certain number of children who are hare-lipped and a certain number who are not, and it is curious that sometimes there seems to be an alternation, every other child being thus deformed. I have known an instance of that in one large family. Of course, where there is a hereditary tendency to hare-lip, it becomes a matter of great importance to take care of the mother's health in the early weeks of pregnancy. And this is more particularly the case if it should happen, as it does sometimes, that the tendency to hare-lip is on both sides of the house. I have myself met with an instance in private practice where both the husband and the wife, who were cousins, inherited from a common ancestor a tendency to hare-lip. They married, and I was called in to operate upon one of the worst cases of hare-lip that I have ever seen. Now really it is almost hopeless to get people to regard these matters philosophically, but I think that medical men, in the case of cousins, particularly, where there is a tendency of this kind, should point out the danger which may occur later on in life. I have here a photograph of a very bad case of hare-lip, and on the back of it is written the history, and I will briefly read it to you. The great-grandmother of the patient, who was hare-lipped, had four children, one boy and three girls. The eldest girl married and had fourteen healthy children, and she also had two hare-lipped children, one of whom died during the operation for hare-lip, and the other, having grown up, married a healthy husband and had this child with hare-lip and cleft palate. Another daughter of the old woman married, and she too had a healthy male child and two female children with hare-lip. The son of the old woman, himself unaffected, did not marry. We have, therefore, distinct evidence in this family history of the deformity being hereditary.

Now we come to the question of the treatment of these cases, and the first point I would discuss is the feeding. A child with a single hare-lip only can no doubt suck, and can therefore be put to the breast, or can have a bottle easily enough given it; but where the deformity is complicated by a fissure of the palate the child cannot suck, it cannot make a vacuum in its mouth, and therefore it is unable to draw the milk from the mother's breast, and it becomes necessary to feed it with a bottle, or with a spoon. A spoon of course answers very well, but is exceedingly troublesome. To feed a child adequately with a spoon really takes the whole time of a nurse, and the better plan therefore is to employ one of the old-fashioned bottles by which the milk can be poured into its throat, and in that way will go down into its stomach and the child will be adequately nourished. It is of the greatest importance that these children should be properly nourished in the first few weeks of life, because if an operation is to be successful the child's health must be maintained.

And that brings me to speak of the period at which the operation should be performed. I have operated as early as a week after birth, but that was under special circumstances, and I do not recommend it. I think the best time for operating upon a hare-lipped child is when it is about six weeks old, and there is the great advantage that the mother by that time is restored to health, and is able to attend to the child thoroughly; and after all there is nothing like a mother's care in the treatment of these cases. Occasionally these children are allowed to grow up much older than that, and the great disadvantage is that if you wait till the child is a twelvemonth old, we will say, of course it is teething, and that is a process which naturally upsets the health somewhat, and makes the condition unfavourable for operation. But in remote districts children are often allowed to grow up to twelve or fourteen before they are interfered with, and we have had recently in the hospital a boy of thirteen, sent up from Wales with an exceedingly bad double hare-lip. The boy must have been a great annoyance to all the pregnant women in the neighbourhood, and I am happy to say that I have succeeded in closing the lip and restoring him to a very fair average appearance.

Then as regards the question of the operation to be performed, I think there can be no doubt as to the modern operation being a great improvement upon the older one. Formerly, we were content simply to pare the edges of a single fissure, pin them together with hare-lip pins, and then to hope that rapid union would take place, so that on the withdrawal of the pin about the third day, union might be complete; and, no doubt, in a certain number of cases this fortunately occurred. But in a very considerable number of cases, when the pins were withdrawn, the union gave way, the lip broke open, and the operation had to be repeated a second time. Now the operation which I advise you to do is this, to pare one side of the lip freely, and to cut a flap from the other side of the lip and transfer it to the opposite side. It is most convenient, I think, to take the longer of the two sides —they are seldom both of quite the same length— and then, having transfixed it with a narrow knife, to cut off the margin of the lip, leaving it attached at the lower border; on the opposite side to cut freely away the edge of the lip, and then to bring the parts together simply with sutures, not pins.

I have myself a preference for using dry chromicised catgut, which I find answers extremely well; but you will find that many surgeons use horsehair, and some silk. The objection I have to silk is that it is rather apt to absorb putrescent matters from the mouth, and thereby inflammatory action is set up, which interferes with the union.

You cannot be too careful in stitching the lip and in putting in plenty of fine catgut sutures, and I would particularly insist upon the necessity of putting sutures at the back of the lip. In the old days, when we put in hare-lip pins, no doubt the back of the lip was drawn fairly together by the pressure of the pins, but if you trust to sutures only, you will find that the back of the lip is practically open, and the discharges will be apt to get in from the mouth, and in that way the union of the fissure will be interfered with. By turning the lip inside out by means of one of the sutures which has been left long on purpose, it is quite easy to put in two or even three sutures at the back of the lip, and they give no trouble because they lie there quietly for two or three days, and then the child either sucks them out, or they come away of themselves, and you do not have the trouble of removing them. Then again, the introduction of the American india-rubber plaster which is made by Seabury and Johnson, has been of great use in the operation of hare-lip, and you will notice that, after I have brought the lip together by several sutures, I always put a piece of plaster on the cheeks, so as to keep them together and to take off all tension from the lip. Of course a straight piece of plaster will do, but it is more convenient to have it cut somewhat like a dumbbell, the two broad ends being attached to the cheeks and the narrow end passing across the lip.

Then having got the lip together in that way, let me say that it is very advisable to prevent the child crying, and the only way that I know of preventing the child crying is by having it assiduously nursed, and also by giving it minute doses of opium. You will find that one of the best ways of giving opium to a young child is to put one or two drops of laudanum into an ounce of dill water, and then to let the nurse give a teaspoonful of that every two or three hours, so as to keep the child a little under its influence. I would remind you how easily young children are affected by opium, and, by thus arranging to give the eighth or twelfth of a drop as the dose, it is quite possible to get the sedative effect of the laudanum without running the least risk of poisoning the child. Then, of course it is very important that the child should be fed, and while the lip is healing it is not advisable to put a bottle to the child's mouth, and therefore you must trust to spoon-feeding with good milk; and let me say that the best combination, I think, for a young child is equal parts of cow's milk and warm water, with a teaspoonful of sugar of milk put into each cup of the mixture. These simple cases as a rule do well, but when we come to the more complicated cases of double hare-lip of course the operation is a more serious one, and the risk of failure is greater.

And first, with regard to the uncomplicated cases of double hare-lip such as that I showed you in the photograph (Fig. 27). There it becomes a question, which every surgeon must decide for himself, as to whether he shall utilise or cut to waste the central projecting portion. In the case which is shown here, I very much preferred to utilise the central portion, for the reason that the two nostrils were perfect, and that the columna of the nose was well-formed. Therefore I contented myself with cutting the central portion to a more acute angle than it was before, removing the whole of the red mucous membrane from it, and then, paring the edges of the outside flaps, I brought them all together and eventually made a very satisfactory lip. But in the complicated cases where there is more or less of a projecting inter-maxillary bone (Fig. 28), it becomes a much more serious question, and I have no hesitation myself in strongly recommending, in all cases where the inter-maxillary bone is loose, that it should be cut away. I know there have been various ingenious proceedings invented from time to time by which it is proposed to bend the inter-maxillary bone back into its place, and thereby, as is thought, to gain something; but I have seen cases which have been so treated and which have grown up in the most unsatisfactory condition, the inter-maxillary bone being still loose although the lip had been brought together over it, and the bone therefore being perfectly useless for the support of teeth, and very much in the patient's way. In fact, in two cases of that kind which have come before me some years after the original operation, I have then cut away the inter-maxillary bone, and I recommend you to do it early in the case. And for this reason: if you remove the inter-maxillary bone of course you take away the incisor teeth, but you will find that the other portions of the alveolus tend to come together and that, as the child grows up, the interval between the two portions of the alveolus is not nearly so large as you might anticipate. In these days of artificial teeth, it is quite easy to get a dentist to fit up some incisor teeth for a child even early in life, or at all events when it has come to puberty, and I am quite sure that artificial teeth fitted in that way are very much more useful to the patient than the natural teeth, which always come out of the inter-maxillary portion irregularly, generally sideways, and give great inconvenience.

Then with regard to the proceeding of removing this inter-maxillary bone, let me say it is a very simple operation, for you only have to nip off the bone with a pair of bone-forceps. But let me warn you there is always a little spouting artery, and therefore you ought to be prepared with a hot wire of some kind, so as to be able to touch the point and arrest the hæmorrhage. It is very difficult to effect this in any other way, but with a needle heated in a spirit lamp it is readily enough arrested. Now an operation of that kind, although not very severe, is quite severe enough for a young child, and it would be very rash to proceed on the same day with the operation of paring the lip. Let the child thoroughly recover; let it have a week or a fortnight, and get into thoroughly good health again after the removal of the inter-maxillary bone, and then it will bear in all probability the remaining operation satisfactorily.

The operation in these cases is complicated by the fact that the piece of flesh which is upon the inter-maxillary bone, and which I need hardly say should never be cut away with the bone, is really the representative of the columna of the nose; and if you are to have a good result you must restore it to its proper position as the columna of the nose, and make no attempt to bring it into the lip. I have seen cases in which that mistake has been made, where the columna has been brought down and put into the middle of the lip, and the result has been exceedingly unsatisfactory. But the proper thing is to turn this little piece of skin and flesh into the position of the columna of the nose, and then to pare the edges of the lip and bring them together in the middle line, in fact to convert what was originally a double hare-lip into a single hare-lip, and treat it accordingly. And these cases, which do remarkably well, account for the fact that it is said from time to time that a patient has had a median hare-lip. Now I believe a median hare-lip is an exceedingly rare thing, and probably has only been met with once or twice. The cases in which there is a median cicatrix in the lip are these cases of double hare-lip, where the parts have been treated in the manner I have described.

Now with regard to bringing the two halves of the lip together below the columna, I think myself the best plan is to save as much of the tissue as you can and to bring down a paring from each side to make a little projecting mass in the middle. You bring the lip together by sutures and you carefully suture together the small projecting portions, which of course must be trimmed up to fit accurately together, and not be too long. All the divided tissues tend to shrink, and therefore the projecting lump in the middle of the lips serves the purpose of preventing that unsightly notch, which used to be so commonly seen after operations for hare-lip.

In all operations upon young children it is of course very important not to lose more blood than is necessary. You will observe how careful we are in these hare-lipped patients to grasp the lip on each side while it is being divided, in order to prevent hæmorrhage from the coronary arteries. As soon as the sutures are put in they prevent any further hæmorrhage, and no harm as a rule follows. But it is well to warn the parent and the nurse that these children have certainly swallowed a small amount of blood, and that blood will go through their bowels and will appear as a black motion within twenty-four hours. I mention this because it is a fact that is alarming unless it is explained, and if beforehand you have told her what is likely to happen, you will avoid any unnecessary alarm on the mother's part. Then let me also say that in the modern operation for hare-lip an anæsthetic is essential. Chloroform answers very much better for children than ether, and they take chloroform remarkably well. I think therefore every child who is to be operated upon for hare-lip should have chloroform carefully administered to it, and so far as I have seen the simpler the apparatus used the better. A handkerchief with a little chloroform sprinkled upon it answers better than any form of inhaler. Of course it may be necessary to arrest the operation in order to renew the application of chloroform, and usually there is no difficulty about that except at just one or two critical moments, when it is better to go on with the operation than to run any risk of failure.

With regard to the cleft palate, which so often complicates these cases, I would merely say that it does not do to operate upon the child too young. For my own part, I prefer to wait until the patient is at least two years old, and then, if it is a suitable case, the operation is fairly successful. But there are many cases of cleft palate which are much too wide to admit of an operation, and yet we find that the effect of the operation of hare-lip is to draw the parts together, and that a case of cleft palate which looks at first hopeless may in the course of three or four years become capable of amelioration.

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Printed by Ballantyne, Hanson & Co.
London and Edinburgh

No. 1.

London, 11, *New Burlington Street,*
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